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ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY
ARISING FROM THE USE OF ASBESTOS IN ONTARIO

CHAIRMAN: J. STEFAN DUPRE, Ph.D.

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Association of
North America.
Conducting the Examination-in-chief

COUNSEL: JOHN I. LASKIN, LL.B.
To the Commission

APPEARANCES: Ms. L. Jolley, Ontario Federation of
Labour.
M. P. Casgrain, Quebec Asbestos Mining
Association.
Mr. J. McNamee, Government of Ontario.

180, Dundas Street,
Toronto, Ontario.
Friday,
August 14, 1981.

VOLUME XXVII

ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY
ARISING FROM THE USE OF ASBESTOS IN ONTARIO

VOLUME XXVII

INDEX OF WITNESSES:

Page

DR. HARRISON B. RHODES	Examination -in-chief	4
DR. GERALD R. CHASE	(Sampson)	
	Cross-examination	74
	(Casgrain)	
	Cross-examination	77
	(Laskin)	
	Cross-examination	120
	(Jolley)	
	Cross-examination	123
	(McNamee)	

INDEX OF EXHIBITS:

EXHIBIT NO. 39, Table 2.	Table to be appended to	61
	Written Statement	

Note: A compendium of articles of Dr. Rhodes and Dr. Chase.
allocated a reference Exhibit No. 39, was not
introduced into evidence.

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THE FURTHER PROCEEDINGS OF THIS INQUIRY
RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Good morning.

Mr. Sampson, I see that Dr. Rhodes and Dr. Chase
are indeed here.

MR. SAMPSON: Yes.

DR. DUPRE: You will be conducting the examination,
that is correct?

MR. SAMPSON: Yes.

DR. DUPRE: And are there any matters that anyone
wishes to raise before I greet the witnesses?

MR. LASKIN: I suppose I'd better raise one matter,
Mr. Chairman, and maybe a second. I gather that Dr. Finklestein
apparently, and I don't know the reason, is not available on the
20th of August. Linda tells me he's available on the 25th or 26th,
so subject , perhaps I can canvas everybody at the coffee break.

DR. DUPRE: Well we will take it that he will be
coming on one of those two days, but on the 20th, I assume we still
have a witness scheduled.

M. CASGRAIN: Dunnigan.

DR. DUPRE: Dr. Dunnigan. All right.

MR. LASKIN: The only other point is, Mr. Chairman,
we have a telephone message from Mr. McNamee, who said that Mr.
Tim Schumacher will be appearing on behalf of the government today,

MR. LASKIN: (Cont'd.) so that if a new face appears at the counsel table ...

5 DR. DUPRE: Oh, I see. In lieu, you mean simply, of Mr. McNamee, and that is Mr. Schumacher, you said.

MR. LASKIN: Yes.

DR. DUPRE: Thank you counsel.

Any other matters?

10 MR. Sampson, do I assume that you will be wishing to examine both witnesses simultaneously?

MR. SAMPSON: Yes, if that's O.K.

DR. DUPRE: So, in other words then, we are now going to have our first swearing in testimony, but it's a double header. Is that right?

15 Miss Kahn please, would you swear in the witnesses, and may I say Dr. Chase, and Dr. Rhodes, that you are both welcome back. Thank you.

DR. HARRISON B. RHODES AND DR. GERALD R. CHASE SWORN

20 EXAMINATION-IN-CHIEF BY MR. SAMPSON


DR. DUPRE: Please proceed, counsel.

MR. SAMPSON: Q. We've submitted an advance copy of a statement, which I hope has been circulated to everyone, and I think the witnesses in a little bit, are going to summarize that statement, just so that we can refresh everyone's memory, at least
25 those who've read it.

I'd like to begin though, by asking a couple of questions, to sort of indicate why we have our double header here today, and to establish for the record, some of the background of these two gentlemen in the area.

30 Dr. Rhodes, I guess I'll start with you. Could you state your current position and employment?

DR. RHODES: I'm the manager of occupational health



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A. (Cont'd.) for Union Carbide Corporation metals division.

5 Q. What kind of responsibilities does that job entail that might be relevant to this, this testimony?

A. Well basically, I'm responsible for the administration of the occupational health and industrial hygiene programme for the division.

10 Q. And I take it that in prior years at Union Carbide, you were also involved in industrial hygiene matters. Could you ...

A. I was involved with the asbestos department primarily, in their monitoring general worker protection, that sort of thing.

15 Q. What, well, let me step back a minute. What is your educational background briefly?

A. I have a doctorate in chemical engineering from Columbia University.

20 Q. O.K., could you sort of summarize quickly, what your experience is in studying, and dealing with the membrane filter method, and perhaps other methods of measuring asbestos exposure?

25 A. I supervised the laboratory that we had on location in Niagara Falls. It was doing membrane filter monitoring both to supplement our own facilities at the plant, the mine and mill, and we did a great deal of monitoring of uses of asbestos. I served the AIANA as chairman of their air monitoring committee, and also with Dr. Chase, was the U.S. industry representatives on the dust measurement advisory panel of the International Asbestos Association.

30 Q. Taking them one at a time, can you tell us a little bit about what the AINA's air monitoring committee has done in the area, and then also tell us a little bit about what the DMAP, the Dust Measurement Advisory Panel has done in the area,

Q. (Cont'd.) particularly with reference to the membrane filter method.

5 A. The AIANA Air Monitoring Committee, it's largest activity, was the set-up and the carrying out of the Round Robin study, that we're going to discuss in greater depth today.

We also have provided information on other monitoring methods besides the membrane filter, but by and large, it's been the Round Robin study on the membrane filter method.

10 Q. And the DMAP.

A. DMAP started about three or four years ago, I think, wasn't it, about four years ago Gerry?

To try to, let me back off a moment. We had an invited meeting in Warmen Steinach in Germany, where we reviewed the membrane filter method, and other alternative methods to
15 measure airborne asbestos concentrations in the workplace. We looked at the way that it was being used in the various countries of the world, and concluded that the need for standardization was very pressing, and the Dust Measurement Advisory Panel took upon themselves the task of doing a fair amount of research work in
20 their own laboratories, and in putting together what is a standardized method for the membrane filter method. This was published as the international reference method.

Q. This is what's known as the AIA reference method that's I believe that was an exhibit of Dr. Trudeau's, or a tab in Dr. Trudeau's exhibit.

25 O.K., Dr. Chase, could you state your, summarize your educational background, and current position?

DR. CHASE: A. Well, my current position with J.M. is file statistician slash epidemiologist, and basically that responsibility for developing and administering the biometry
30 programme, and perhaps the best way to describe the biometry programme, is to go back into my academic and professional background. I have a Ph.D. in mathematical statistics from

5 A. (Cont'd.) Stanford. While I was at Stanford, I was supported by a public health training grant, which got into the health applications of statistics. When I left Stanford with my degree, I went to the University of Missouri, where I took a joint appointment with the Department of Community Health and Medical Practice, in the medical centre, and the Department of Statistics in the School of Arts and Science, and then subsequently, I took on a research role in the Space Sciences Research Centre, at the University of Missouri.

10 And then in the early seventies, I took a leave of absence for a year, and was visiting scientist in the Biometry branch at the National Institute Environmental Health Sciences.

15 Then subsequent to that, I joined Johns Manville as the file statistician epidemiologist, with responsibilities then for the development basically of the health and environmental data base, with the idea in mind of current uses of those data, as well as laying the foundation for future uses of those data, in health matters.

20 Q. I take it you've been involved studying the membrane filter method for five or six years. Could you sort of summarize what your background experience has been in that area?

25 A. Yes, the first thing that I did with the membrane filter method, was to look at it analytically, to see with particular reference to the counting rule itself, and whether or not it was an unbiased counting rule, and also to the estimates of the measure of the uncertainty that's coefficient of variation of the membrane filter method.

30 An outgrowth of the first look at it was a paper that was published, I believe, professor Cooper, and Dalman and myself, at Harvard, which corrected the bias in counting rule. It was a technical point, a few technical points, and the rules as to when you did or did not count a fibre, and that has led to the

5 A. (Cont'd.) correction of that by NIOSH, in
their counting rules. And at the same time, the investigation of
the uncertainty inherent in the measuring, in the membrane
filter measurements themselves, through in-house studies with
J.M. industrial hygienist looking at empirical information, as
well as reviewing the literature. That then led to my participation
invitation, and participation of the first colloquium at Warman
Steinach, and subsequently then, as a member of the Dust
10 Measurement Advisory Panel, and of course on the AIA Air Monitoring
Committee as the statistician in that connection.

Q. That's with respect to the Round Robin study.

A. That's correct.

15 Q. As , in your activities on the DMAP led to the
AIA reference method, perhaps you might wanna either one of you
can answer this, but can you give us some idea of what kind of
reaction in the international governmental, or scientific
community, that AIA reference method has received?

20 DR. RHODES: A. I think I would be remiss if I
said it hasn't been well received. It's my understanding the
Canadian people are studying it. Graham Gibbs had a programme
going. It's been adopted in Germany, and for the international
affiliates of the German companies. It has been discussed
extensively with the International Standards Organization. The
primary points of discussion, are not the method per se in a
sense of the collection of the sample, or in counting in general,
25 but in the manner that certain fibre configurations should be
counted.

30 It looks, we are hopeful anyway, that the ISO
will adopt major portions of it, with some minor modifications in
the count. By and large it's been well received as a standardized
approach to the monitoring method.

Q. O.K., that sort of completes the preliminary
questions I wanted to ask. Why don't you two gentlemen go ahead

Q. (Cont'd.) and summarize the statement that's been submitted already?

5 DR. RHODES: A. I guess I'm on first.

What I would like to do, we elected not to just sit here and read our statement, but talk somewhat informally about the membrane filter method, and present some of the background in a simplified way, on how we set up the Round Robin, and what we intended to accomplish with it, and then have Gerry go on, and
10 present some of the results.

As has been indicated by the earlier questioning, membrane filter method in one form or another is used largely throughout the world, in the measurement of occupational asbestos exposure. I would refer you to a table in the first
15 Warmen Steinach report, which lists, summarizes the manner in which it's used in a large number of countries.

Q. Excuse me, are you referring to the first colloquium?

A. First colloquium at Warmen Steinach yes, I'm sorry. Is there a problem there?

20 Q. I'm seeing some confusion here. Do we have any way of identifying that? That was one of the material that we sent to you in advance. It's the first dust colloquium.

Dr. Is that the 1977 colloquium?

A. I believe it is. It's the first colloquium sponsored by the DMAP. I'm sorry Art, I didn't mean to confuse the
25 issue. It's a minor point. It gives a nice illustration of the countries that are using the method, and the manner in which they were using it at that point in time. And since this is the method that is used to measure workplace exposures, most of the standards, governmental standards, are also related to exposures as
30 measured by the membrane filter method.

Now I don't think there are any illusions that the

5 A. (Cont'd.) membrane filter method is the
greatest, it meets all the requirements, it's a very fine method,
or anything of that nature, but it does have some very strong
reasons for use, and basically, it's very simple, relative to
some of the alternative methods. It can be set up and operated
at a reasonable cost, and it's suitable for routine measurements,
where you have large numbers of samples, and in the workplace
standard for asbestos, the number of samples that need to be
10 run, is virtually staggering.

And the epidemiology studies that have come out
in the last five to ten years, are also generally keyed to
exposures as measured by the membrane filter method. It is pretty
well in place, widely used, and related epidemiology.

15 Now there are some admitted limitations. It is
difficult to distinguish asbestos from other moneral particulates
which have dimensions, long narrow dimensions. It also operates
at a very low magnification, so that it misses certain long thin
fibres, and it also doesn't pick up, you don't count the short
fibres.

20 It's also difficult if you've got a mixture of
asbestos types, to distinguish between chrysotile, and
crocidolite for example. Now this is not a bit problem in most
industrial operations, which use only chrysotile, but there are
some which use both.

25 So even with its limitations, I don't know of
any better way to monitor workplace exposure, than the membrane
filter method. I think it's important that we recognize its
limitations, but I would be very concerned if we suddenly
switched to some alternative method.

30 Now we've talked about the membrane filter method.
I thought I would just say very briefly, how it works. We were
hoping to have a pump in here, but we were unable at the last

5 A. (Cont'd.) minute to come up with one. It's helpful to think of the method, as having two parts, the collection of the sample in the workplace, and the evaluation of the sample after you've got it collected on the filter.

10 In the collection step, the membrane filter, which is a little round paper disk, is put in a holder, and suspended in the breathing zone of the worker, in this general area. A piece of tubing, plastic tubing, goes over the shoulder, and is attached to a vacuum pump, and the pump is calibrated to pull air through at a set rate, generally about one point seven litres per minute, and it's placed on the man, in a manner that tends to be least interfering with his normal work. It's turned on for a measured period of time, and a known volume of air is drawn through. Now this step has not received very much attention in the literature on the method, and it can be very critical. You're trying to get a representative sample of what the man is actually breathing.

20 Once the dust is collected on the membrane filter, you take the filter to the laboratory, cut a small wedge out of it, and there are various ways of making that wedge transparent, but you mount it on a microscope slide, make it transparent, and then you examine it under a microscope at about five hundred magnification. The microscope, you have a little graticule, it's called, which projects on the slide as a known area, and you move that graticule at random, and by certain counting rules, you count the number of fibres you see, usually a hundred fields at low concentrations. It may be less than that at higher concentrations. So that you know how many fibres you have found in say a hundred fields, or a certain area of the slide, and you know the total area of the slide, so you can calculate the total number of fibres you've accumulated on the whole filter, and you know the concentration of air that you , the quantity of air that you've

5 A. (Cont'd.) drawn through it, so that you know the number of fibres per cubic metres, cubic centimetres of air, which gives you the concentration that you're looking for.

Q. Dr. Rhodes, about, just to get an idea, how long does it take to count one of these for a laboratory analyst to count one of these filters?

10 A. It will vary considerably on the number of fibres, on the filter, the number of, amount of background dust and that sort of thing, but an experienced counter, I would say ten to thirty minutes is a representative figure on a counting. That's after it's cleared and set on the microscope.

15 It had been recognized for lots of years that this counting of fibres on a filter, is as much an art as it is a science, and that there was a great deal of variability in the method. There had been when we became interested, four or five years ago, in doing a more comprehensive study, there'd been a substantial number of studies reported. The problem was basically they had looked at the counting step only, after the fibres are on the filter, what kind of variability do you get in fibre counts. They had not really considered in any depth, the
20 important problem of obtaining a representative sample of the workplace air onto the filter, before you started to count.

The first attempt to do that really, that I know of, was the J. M. study, that Gerry Chase has mentioned earlier. And we more or less patterned our study on that approach, and
25 invited, we put together a protocol of what we were going to do, and we invited about two hundred laboratories that we were able to locate in the United States to participate. We wanted about forty or fifty, and we got forty-six laboratories that volunteered in this fairly extensive programme.

30 DR. UFFEN: Q. Any Canadian?

DR. RHODES: A. I can't answer that question, because we set up for them to be totally anonymous. We wanted to

5 A. (Cont'd.) get government laboratories, we wanted to get industry laboratories, we wanted to get consulting laboratories, so everything went through a coordinator, who was the only one that knows who the participating laboratories are.

DR. UFFEN: Do you know who the invitations went to?

10 A. the invitations went out to basically to the U.S. laboratories. I would not say that there was not a Canadian company or three in there, but one of the requirements to participate, was that they were using the NIOSH membrane filter method as it was in place at that point in time, so that the Canadian laboratories basically were using some variation there-of.

15 DR. CHASE: Harry, as you were saying, You couldn't say for sure, I shook my head, Yes, and the reason for that is that I do know that there was at least one, because it was the J. M. lab that was participating.

DR. RHODES: No, we thought it was important to attract people, not - to give them anonymous participation.

20 Out of those forty six laboratories, about half of them were laboratories that were active in the U.S. NIOSH Proficiency Analysis Training Programme, PAT programme, which is the, it's run by NIOSH, and it's a standardized sample exchange programme, that you count a certain number of samples every month. Again, the problem with that is the filters are prepared, and you're looking at counting only.

25 Now to illustrate the design that we used in the Round Robin laboratory, let me hit briefly, an illustrative position. Let's say that you have a large group of competent laboratories. I won't define competent, but experienced competent laboratories, and you had a workplace where you wished to have the worker exposure measured, and you took all these laboratories
30 and put their names in the hat, and pulled one out, and had this

5 A. (Cont'd.) laboratory come in and make a measurement, and you would get a certain report, result reported. Now if you had happened to pull one of the other laboratories' name out of the hat, you might get exactly the same number, but the chances are very high that you'll get some other number, and if you repeat this, or do it, we'll get another laboratory, you're going to get a series of numbers which vary because of the uncertainties in the method.

10 These uncertainties really, let me back off a moment. It's this variability that you're really concerned with, when you're trying to determine whether you're in compliance, or when a government laboratory measures you to determine whether you're meeting government regulations. The concept of a - the independent collection and measurement of the workplace concentration. Now these differences, and I'm going to mention this, because we talk about it quite a bit, later, these differences are due to two principal types of errors. We all know that if you make a measurement, you're not going to get exactly the same number every time you make a measurement. There are a number of small random errors which cause differences in results.

20 You also have a type of error where let's say you have miscalibrated your graticule area, and that every time you use, make a count, you have a constant systematic error, and this type of error in a laboratory, it will be repeated, it will be buried in all of the results.

25 What we try to do in our programme, is come as close as practical to the situation, where you had an independent laboratory come in and make a measurement, but it really wasn't practical to try and set up a programme where we had randomly selected laboratories to come in and make simultaneous measurements.

30 What we did, was have laboratory collecting the samples, take a simultaneous pair of samples on the man, and

5 A. (Cont'd.) continue this, so that we had a full shift of paired samples. Now this meant that any errors, systematic errors which were present, if the laboratory had miscalibrated their pumps, for example, this type of error would not appear, but all the other sources of error that were left, would impact on the variability of the results that we were obtaining.

10 Using these filters, we obtained three basic types of count. The simplest, - a number of the laboratories, and this was strictly voluntary, and unfortunately, it was not all of the laboratories, where there were more than one counter, we asked them to make repeat counts on the same filter. This gives you a measure of the random variability within the laboratory, 'cos they're all using the same equipment. They've all
15 been trained pretty much by the same people. They're all calling their shots pretty much the same way.

20 The next level was to take the filter, and have one laboratory count it, and then send it on to a second laboratory, so that we got two laboratories providing counts on the same filter.

25 Last, and the most important one, which was really the basic objective of the study, the laboratory which collected the samples, counted one set of the filters. The second set was sent to a randomly selected second laboratory. This doesn't give you a great deal of information on each pair, but we had somewhere in the order, the number escapes me exactly, about
30 nine hundred paired sets, eighteen hundred filters, and in the neighbourhood of twenty-five or twenty-seven hundred total fibre counts made up the data base for the entire study.

30 What I will, what I want to do to finish up here, is take figure one from our testimony, and kind of summarize what I've been saying here.

Notice first at the left, that we have divided the

5 A. (Cont'd.) process membrane filter method into the two parts that I've indicated. The sample collection step, and the variability that that step introduces, and the sample evaluation step, which is the counting of the fibres after you get them on the filter, as the second part of the process.

10 The simplest counts, as I indicated, were when different counters in the same laboratory, counted the same filter. We've referred to that as within laboratory within filter comparisons for intralaboratory intrafilter.

15 Now since the equipment procedures and that sort of thing should be fairly well standardized within the laboratory, what you're looking at there are the small random errors in the counting method, and the basic fundamental poisson error that is inherent in this kind of selection of small samples out of a larger sample.

20 The second area where we use the same filter, and had more than one laboratory counted, will pick up these systematic errors that are occurring between the laboratories, so that you've got a measure of the total variability in just the evaluation of the filter.

25 When we go on, and look at the paired filters, the counting or the collection step, you've introduced the problem, the errors of trying to get a representative sample out of the dust cloud. Now again, these are random errors, because you had a single laboratory collected both filters.

30 Finally, the error which we were unable to address, the systematic error in the sample collection, I really don't have any basis, but my instinctive feeling is that the problem, the random error problem of getting a sample out of a varying non constant dust cloud is the major problem in that step, and that the systematic or persistent may not be a major factor, but I offer that as an opinion, rather than something that I can support.

5 A. (Cont'd.) Well, that is all that I have. I'd be glad to answer questions on it here if they wish. Is that all right Art, or Gerry go on and complete the resume?

Q. Why don't we have Gerry, unless someone has something they want.

DR. UFFEN: Well, I'd like to come back to it.

Well, I could ask him now, while it's on our minds.

DR. DUPRE: Please proceed, Dr. Uffen.

10 DR. UFFEN: I was interested in your emphasis on the two aspects, collection, and evaluation afterwards, because you are quite right, we have lots of people tell us about the difficulties of evaluating, but not very many have discussed the difficulties of the actual collection. You used the example of the calibration. Could you spend a moment or two though, and talk to
15 us a little bit more about what things affect the collection procedure other than say the calibration.

I'm thinking about comments you've made in here about temperature, the pressure. I'm also curious about the effects of humidity if there are any.

20 DR. RHODES A. The principal problem that I see with the collection step, are inherent in the nature of the dust cloud that you're trying to get a sample out of. Now if you are trying to sample a point source, for example, a man sawing an asbestos cement pipe, a man dumping bagged asbestos into a hood, where you're generating the fibres in a relatively narrow area,
25 and you're going to have a substantial concentration gradient from the source to where, into the room, and where that man stands, how he handles, whether he's right or left handed, how he does the job, will have a major impact on the concentration that you collect on the filter.

30 Now in contrast to that, you may have a room where there are a large number of diffuse sources, and the whole

5 A. (Cont'd.) room is at fairly close to the same concentration. In that case you see, it really doesn't matter significantly where you put the filters, where the man stands, and that sort of thing.

The point I'm making is that, I think the nature of the dust cloud, and the relationship of the filter to where you sample from in that cloud, is the critical factor. Now the temperature and pressure ...

10 DR. UFFEN: Q. Just before you leave that, who would be the best judge of where to measure the cloud?

A. This is a matter of basically, industrial hygiene technology, professional judgement I think is the only word I could use.

15 Q. And would most qualified industrial hygienists know enough about this machining, the sawing, whatever it is, to know precisely where to make the measurement, or would the workman know best?

A. Well, I usually try to observe what he's doing, and make a judgement from that consideration.

20 Now let me add that there is another study that started about the same time that we did, and they're even slower than we are. I believe it's NIEH S rumor around (Ph) NIOSH. It's not an asbestos study, but they are, they have four or five sample collecting pumps on the same worker, and are looking specifically at this problem of how do you get a representative sample, and we have not been able to get a copy of this. It's in review. The scuttlebutt if I may use the term, is that their numbers, their results don't look much different than ours, and that it is an important source of variability in the workplace measurement.

25
30 Now the other items that you mentioned, are corrections.

DR. UFFEN: Q. Can you give us some idea of whether they are big corrections, or really technicalities?

5 A. Well, it depends, if I calibrate at Niagara Falls at basically sea level, and go out and take samples around Denver, it's a correction factor that's worth going through the procedure, the procedures to correct for it.

Q. Ten percent, twenty percent?

10 A. It's that order of magnitude if I recall. It's better to calibrate in the same area that you, you know, basically it's the same temperature and pressure conditions that you're going to operate at.

Q. Would it be safe to say then, that normal weather pressure variations would be probably unimportant?

15 A. I would say relative to all the variability that you've got in the method. They should be taken into account, you know, calculate them, but they're minor corrections on a larger variability.

Would you agree, or disagree with that, Gerry?

DR. CHASE: As a generalization.

20 DR. UFFEN: Q. At mile high Denver, I guess they talk about a pressure difference for a mile, would be ten times any - in a storm. What about temperature and humidity?

25 DR. RHODES: A. Well again, temperature, you have a correction factor. You're calibrating in a laboratory at seventy fahrenheit thereabout. If you go out at ten below zero out there somewhere, in a very cold area, you should make a value of correction, that's the ratio of the absolute temperatures, that kind of a correction.

Q. Would a ten or twenty degree fahrenheit, ten degree fahrenheit, be significant, or not?

30 A. My comment on that is, I don't think you can read the volumes well enough to worry about the kind of correction

A. (Cont'd.) you get from a ten or twenty degree fahrenheit temperature difference.

5 DR. UFFEN: I have in mind one specific problem. In many plants, there is a room called the drying room, where temperatures may be quite significantly different, and humidities may be quite different. This is what I, would the instruments have to be calibrated more frequently for such a measurement?

10 A. Well, the point you want to remember, is that what you're doing is looking at a gas volume correction, and those can be made. It's not that difficult. You're looking at a temperature and pressure effect on volume.

15 Q. Provided that somebody records it though, and pays attention to it. Does the procedure now ask that the temperature be recorded at the time the measurements are made?

A. The NIOSH method calls for a correction, but I don't believe one of the required, I would like to pass that question. I'd have to check it. We normally will record temperatures and pressures.

20 Q. You just suggested that the variability of the volume would probably swamp these errors. Could you expand on that?

25 A. Well, theoretically, you can measure the volume plus or minus ten percent, when you calibrate it but the pump, the pumps that are widely used, the MSA, and that type of pump with a pulsation type vacuum drive, that little ball in there bounces, and as your filter loads, you begin to change the pressure drop, and as your battery runs down, you begin to draw a little less air, so that you're making corrections. You're trying to read a rotimeter ball that's bouncing up and down, and again, it's a personal opinion. I think if you can get ten percent, you're gonna be doing very well.

30 Now there are some new pumps which have critical

5 A. (Cont'd.) flow orifice type things, which adjust to give you a constant flow, and they also record whether you had a flow upset, so what I'm really saying that the pumps that are in wide use now, are difficult to read, to get a very very accurate flow.

The new pumps are much improved in that area.

10 DR. UFFEN: Q. Is it possible to deliberately cook it, so that you either increase or decrease the rate of flow, and that that would be undetected?

A. You mean the person who ...

Q. Somebody of a devious turn of mind, for whatever reason, deliberately produces a bias, a systematic error.

15 A. We've had that problem rather extensively with some of our miners who are pretty independent types, and the pump that we're using now, the new Dupont pumps, have a little electronic gadget in there that will tell you if the flow has been tampered with, and after we caught a couple of them doing it, they've got to the point where they now know that they can't do it and get away with it. We have much better results. These are the
20 new pumps, and also, when I'm taking samples, I generally like to be right there, and can watch.

Most of the people that you sample, are, once you explain what you want, what you're trying to accomplish, the meaning of it, will be quite cooperative, but there are a few who will deliberately tamper with what's going on.

25 Q. Now on the other side of the coin, is it possible to dicker with the calibration that's been done in the laboratory, deliberately, and have it undetected?

30 A. Normally, we calibrate before we go out, and we check when we come back in. Now the pump, the way you set the flow, what you do is set, when you calibrate, you set a series of readings, and measure the corresponding flow rate, and then you set the little screw, to give you the proper reading, to give you

5 A. (Cont'd.) your one point seven litres per minute. Now if somebody adjusts that screw. you're going to see the ball, the flow rate change, so if you're watching your pump, you should by making minor adjustments to hold that flow rate dust.

DR. UFFEN: Q. Well, am I right in this conclusion? If a laboratory inadvertently or deliberately monkeyed with the basic calibration, in a Round Robin study, that would be revealed?

10 A. We would, by using a lot of laboratories, the component that we're adding, is systematic differences between laboratories, or whether it was deliberately or accidental, if it were present in all of their results, we would see what kind of range you might get in that type of thing, by have it looking at a lot of laboratories.

15 In other words, the systematic variation between laboratories have because a randomized variable in a study of this nature.

20 DR. CHASE: Excuse me. Well just in terms of the calibration of the pump, since the same laboratory was collecting the simultaneous samples in order to have had an effect in terms of the comparisons we were making, it would have been necessary for them to set one pump one way, and the paired pump another way, because if they were systematic in their calibration of the pump, it would be washed out, because we're comparing the two filters, and if the bias was there, it was there for both pumps.

25 DR. UFFEN: Q. Then would I be safe in concluding, that if you want to have an inspection system that could check for this type of systematic error, the only way to do it is to have independent measuring equipment operated by an independent observer?

30 DR. RHODES: A. To be absolutely sure, you've got to have everything independent.

Q. So you'd send in an inspector with his own equipment, calibrated in his own laboratory, as an independent

DR. UFFEN: Q. (Cont'd.) check on the Round Robin.

A. I'm not sure I understand what you're driving at.

Q. I guess I didn't put it very clearly. If some time in the future, a system is in position, and they're using it for regulatory purposes, and there is any suspicion that there are, some of the systematic errors are not accidental, the only safe way, would be to send in an independent observer, with independent equipment, to detect.

A. Well, that would be the surest way.

I think you're over-emphasizing deliberate tampering with equipment, but again, that's opinion.

Q. All right, I suspect that I am, but we have to think ahead, and a regulatory system based on a measurement should really take into account its tamperability.

A. Mmm hmm. All my opinion was, that from what I've seen, there's very little of that goes on.

Q. It's not a problem, All right.

MR. SAMPSON: Q. Dr. Rhodes, before Dr. Chase goes on, you were talking a little bit ago, about how the laboratories who participated in the study were selected. I wonder if you might elaborate a little bit on what the criteria were for participation in the study.

DR. RHODES: A. All right, first of all, they had to be actively engaged in workplace monitoring of asbestos, and one of the questions that they were asked was, how many samples they had counted in the previous year. They had to agree to follow the NIOSH procedure, and we sent them a copy of the one that was current at that time, and basically, those were the criteria, that they be actively engaged, and that they follow the procedures.

Q. One other question. You mentioned that the time for a laboratory analysis of a sample based on the membrane filter

Q. (Cont'd.) method, generally will range under thirty minute, ten to thirty minutes, I think you said.

A. Mmm hum.

Q. Do you have any information on what the cost of that analysis might be on the average?

A. If you contract, if you use an outside laboratory, they generally charge around twenty or thirty dollars for that kind of a sample.

Now if you're doing it in-house, why that's an accounting problem. I might add incidentally Art, that that twenty or thirty dollars is the counting. If you have to hire somebody to go out and collect it, you're talking four or five hundred dollars a day, plus travel and expenses.

Q. But a lot of times, that collection activity can be done by in-house personnel.

A. It is, it can be done, yes, by in-house personnel.

Q. O.K.

A. I just wanted to let you know that there were other costs involved in monitoring.

Q. Dr. Chase.

DR. CHASE: A. Any discussion of the precision and the inherent uncertainty in a measurement such as this, can't really take place without using terms such as coefficient variation, and competence intervals and that sort of thing, so I'd like to, with your permission, just discuss those concepts so that hopefully it will lay the groundwork for the presentation of the study.

Statisticians are in business because there's uncertainty in measurements, and if every measurement we took, gave us an exact number, then we'd have to look for another profession. So the concept of there being a single person

5 A. (Cont'd.) measuring something two times in a row not getting exactly the same measurements, or two qualified people going in and measuring something that they're not going to agree the same way, the fact that there are inherent errors, is just a fact of life. So when we, as we consider in any given situation, all of the possible values that one might get, you have a spectrum of possible values, which we call a distribution, and in order to describe that distribution, we like to talk in 10 terms of some sort of central tendency, or which the common thing we use for that is the numerical average, the mean.

Another thing we like to do, is to give some indication of how spread out the data are.

Could I ... (REPORTER: Reference to blackboard.)

15 MR. LASKIN: You certainly can.

DR. CHASE: A. The sort of thing I'm talking about here, is if I can draw on this scale, simply this represents the range of numbers I'm getting. It's in inches, it's in pounds, something of that sort. When we consider all the possible values that one might get, you often, you're going to get a spread. Sometimes it looks like this, other times it might be spread out 20 like that, other times you get two peaks in it; it varies.

And in order to describe the numbers, it just, we don't often walk around with graphs in our back pocket. Quite often, we like to describe a set of numbers by one or two numbers. We describe a defensive line quite often in football, by 25 talking about the average weight of the line, even though of course, they vary across that. We talk about averages quite often. But the reason we want to get another measurement in there, is we'd like to distinguish between the situation where they're spread out like this, or they're all in there a lot tighter. So, comparing this curve with the peak on it, there 30 might be the same average value for those two distributions, but they differ, so we need some measurement of how spread out they

5 A. (Cont'd.) are, and in order to measure that, the most commonly used measurement of spread, is the variance, or the square root of the variance which is the standard deviation. It's not necessary to know how we calculate it, it's just that if you have a set of numbers, one is closer together than another, the one that's closer together will have a smaller standard deviation than the one that's more spread out.

10 Well the coefficient of variation is , takes that one step further, and it talks about the relative spread of the data, and that is, it's taking, we generally in most texts we use, when you talk about the mean of a distribution, most textbooks will use a Greek letter, and mu is commonly used, and for the variance, we talk about sigma squared, or when it's to the standard deviation, which is sigma, so the coefficient of variation, is how much is it spread out relative to where it sits on this absolute sense.

20 So the coefficient of variation is simply the standard deviation divided by the mean, and while it's not necessary to know absolutely what that is, perhaps an intuitive example; if we were to measure the length of the table, and have a number of people measure that length, we would not probably get exactly the same, depending on the accuracy of the equipment we were using. But if we were to measure it in terms of inches, we would get one type of spread, if we were to ask them to measure it in terms of centimetres, we would get another kind of spread, even though they're talking about the same length. So the inches and the centimetres will differ, but if we were to compute a coefficient of variation for the inches or the centimetres counterpart, the coefficient of variation would be the same. So it sort of becomes a dimensionless measure, which tells us how much those data vary relative to where they're sitting on this absolute scale, and the reason for going into that in so much

5 A. (Cont'd.) depth, is that that's the most commonly used parameter in describing the environmental data or the industrial hygiene measurements.

DR. MUSTARD: Q. Can you say again what you did to calculate your coefficient of variation? It's the mean over the standard deviation.

DR. CHASE: A. No, it's the standard deviation over the mean.

10 Q. Can I just ask you a simple question? Since you're measuring fibres, why wouldn't you just use standard deviations? What's the advantage of doing coefficient of variation when you're staying within a standard unit of measurement, in which everybody understands, and no two standard deviations, sort of everything's, ninety-five percent of your data's within that?

15 That's a dimension most people are familiar with in my field.

20 A. Your question is good. If I can't really get, other than the historical, that's the way in order to relate it to the way everyone has done it, it's necessary to talk about coefficients of variation, but it's certainly, your point is a valid one in terms of intuitive understanding.

Q. I'll ask a further question. Ninety-five percent confidence limits entered, calculated to the standard deviation in the table that's appended to your article,

A. I'm sorry.

25 Q. The ninety-five percent confidence limits which you show in table one, those are calculated from the standard deviation.

A. That's right.

Q. O.K.

A. That's right, yeah.

30 DR. UFFEN: Q. Could I make an observation here?

5 DR. UFFEN: Q. (Cont'd.) We don't just measure fibres we measure fibres per ml , and to accomplish the variation then would take into account the variability in the flow rates, if there is any.

DR. MUSTARD: Well, the mean of the standard deviation if that's ...

10 DR. RHODES: A. Could I comment on that? Actually we do. The coefficient of variation is a convenient way to correlate, if I may use the term, or to present a generalized picture of variability, but when you actually get to a situation where you're calculating confidence limits, you come back, you back out the standard deviation and use that to get your confidence limits.

15 DR. CHASE: A. You're actually getting into one of the things that I was going to get into.

DR. RHODES: Oh, I'm sorry about that.

But the gentleman here is correct. You use the standard deviation when you actually get around to calculating confidence limits.

20 DR. CHASE: Yeah, well, I know you do that, but I just didn't want to, I wanted to ease into it rather than leap.

DR. RHODES: I'm sorry about that.

25 DR. CHASE: A. Well, the next thing of course, is we then, that as a unit, isn't going to do us any good because what the bottom line that we're looking for here is, we do have a final measurement in fibres per ml, and we'd like to know how much uncertainty can we place on that number based on what we know about the measurements that we took, and so that's where then this coefficient of variation is then used, using the appropriate manipulations, to arrive at this uncertainty range, that we're going to get for the final number, the fibre count, the fibre per
30 ml count. And that brings up the concept of a confidence limit.

5 A. (Cont'd.) One way of introducing that, that concept, is to consider an unrealistically simple example first. Suppose that I were to measure something with a measuring technique that was not perfect, but I did know that it wouldn't be off by more than two units. So whatever number I get, I can't be sure that I'm measuring exactly, but I can be sure that it's no more than two units off.

10 So if I have that situation, (Is it picking up O.K.), I'm always within two units, inches, pounds, whatever it is, and suppose I happen to get a measurement of ten, then if I get a measurement of ten, knowing that I'm always going to be within two units of whatever this is I'm really trying to measure, then I can state with absolute certainty, that it's somewhere between eight and twelve, because I'm never going to be away from it by more than two inches, or two pounds.

15 Now let's extend that just a little further, and say I have a measuring technique that is pretty good, and most of the time, I'll be within two units, not all the time, but most of the time.

20 Therefore, if I get a measurement of ten, I'll say, well, I'm pretty sure it's between eight and twelve, but I can't be absolutely certain, because of this thing that I said, most of the time, I measure within two units of something.

25 All right, now how likely am I to be more than two units away from it? That's the uncertainty in the final statement I make, and that's the thing that dictates the level of confidence that you talk about, when you construct the confidence interval.

30 If I stand a five percent chance of being more than two units away from this thing I'm trying to measure, and I give an interval such as that, then I stand a five percent chance of being wrong, or conversely, I stand a ninety-five

5 A. (Cont'd.) percent chance of being right,
and that's a ninety-five percent confidence level. And it's this
not having the measuring technique, whatever it is I'm trying
to measure, that is subject to error, and I accept that, but
what I want to do, is have a handle on that error, so that the
statement I make, will not be made with absolute certainty, but
I can control the chance that I will make an incorrect statement.

10 And it's this trying to measure something, and
then put a range on the values that we could reasonably expect,
from the value that we got, that leads us to this whole thing of
a confidence interval.

Now where's that er ... (REPORTER: reference to
blackboard eraser.)

15 If I can come back to that. If we, relating this
now to the types of measurements we're talking about, the
hypothetical thing of a number of people measuring the same
workplace environment on a specific worker, there are a collection
of values that one might get, and we can represent those by a
curve, and just for convenience, I'll draw a somewhat symmetric
20 curve, for the purpose of illustration, so the height of the curve
here, represents the chance that one would get that value, and
if we wanna put on this scale, we can put fibres per cubic
centimetre, someone measuring a particular work environment.

25 Then if one person comes in and measures, they
might get a value here, another one might have obtained a value
over here, or the whole spectrum, but it's much more likely that
we get it where the curve is higher, 'cos that represents the
higher likelihood of doing that.

30 So in the middle here some place, sits this mean.
This is the parameter that we're looking for, and all the others
just the error that's inherent in the measuring technique.

Now what happens is, if you might suppose for

5 A. (Cont'd.) example that we take the upper portion of this curve that say has two and one half percent, so be above this point just two and a half percent rise.

There's another corresponding point down here, where two and a half percent of the values will fall below there.

10 Now what happens is, in the actual construction of confidence intervals, when I was talking about, and for convenience let's take the ninety-five percent confidence interval, when do you make the incorrect statement, and when do you make the correct statement?

15 Well, you're going to make the incorrect statement, when you fall in one of these, when you happen to get the possible but not as likely extreme values from this distribution. So when you're far away from the true value, you're gonna make the incorrect statement. When you're in the middle, you're gonna make the correct statement.

20 Now of course, if you take a single sample, you don't know where you were relative to this thing, because you don't have the whole distribution, but if I as a statistician, go through life constructing ninety-five percent confidence intervals for people, then there's going to be a tally sheet up in the sky, and when I got up there, hopefully, they can look at my track record, and if I was doing things correctly with my ninety-five percent confidence intervals, then ninety-five percent of the time, I was correct with the interval I gave, and five percent of the time I was wrong.

25 Most of the time, ninety-five percent confidence intervals are used most of the time. In practice, although it's not always, there's nothing magic about it, the biggest convenience there is that if you look in most textbooks, you'll find that there's certain values that are tabled. There's something that'll allow you to a ninety-five percent, there's

30

5 A. (Cont'd.) something that'll allow you do a ninety-nine percent, and something you do a ninety percent. That's the convenience of the tables.

The situation should dictate the level of confidence that you wanna use.

10 All right, now that's the situation that existed when we draw a curve like that, and we talk about all the possible values one might get, that's the sort of information that we don't have. We as the person who's taking the sample, that's the information that the recording angel has, when they're making up my tally sheets.

15 If we take an actual measurement, we come up with one number. So we end up with a single value, say one point two fibres per cc. Now the thing is, when somebody takes a measurement, and arrives at a value of one point two fibres per cc, what they need to ask in order to construct this confidence interval, is it certainly could have come from something that centred there. It also might have come from a curve that was down here, and it tended to be up in the high side of the possible values you might get. It also might have come from a curve that's sitting up here, and it represents the low value in the total distribution, and the thing is that we don't know whether it came from this kind of a situation, whether it was a low value on it, or whether it was a high value on it. So what we need to do then as statisticians, is to consider all the possible curves that
25 might have given such a value, and what we do, is we say, let's take all of those that could reasonably have given us this number, and what's reasonable, well that's again, dictated by the level of confidence that you happen to select.

30 If you do a ninety-five percent confidence, then you use that argument that we had just now. We took those two ends of two and a half percent. So the actual construction of the

5 A. (Cont'd.) confidence interval then considers all the possible curves that might have given you the measurement, and picks the collection of those that are quote, Reasonable, unquote, reasonable being defined by the level of confidence that happened to be selected.

10 Now when we talk about what reasonable curves one might have, that might have given rise to the number you were getting, at this point, I'd just like to put a footnote on what I'm saying, and that is, I've been drawing curves like this, but in actual practice, we don't know what the curves look like. In other words, are the curves that I'm getting nice and symmetric like this, or do they, are they more bunched up at one end, and trailing off, or do they have those two blips on them, like that one I drew before?

15 Quite often, we're not in a position of knowing what type of curve we're sampling from, so what we do then, is what, we would like to develop procedures, statistical procedures that would not be sensitive to the form of the curve. In other words, I'd like to make what I said before, is in my example 20 with the tally sheet in the sky, in order for me to be correct ninety-five percent of the time, I'm going to have to, the theory behind what I do, is going to have to be appropriate.

25 If I make an incorrect assumption, then I might be wildly off, and that's when they are going to put a big red cross across my tally sheet.

30 So the term that's used for this in statistical jargon, is, Robustness, and the best way to do that is perhaps give an example.

If you're going to do a particular task which requires a screwdriver, it's best to have the screwdriver that's exactly the right size for that particular screw, or on the other 30 hand, if I'm, I would like to do something that I wanted to cut

5 A. (Cont'd.) something with, a screwdriver generally isn't the tool I'd want to use for that. So tools are designed for specific tasks, but if I'm going off on a wilderness trek some place, and I might have a need for some tools, I can't carry my toolbox with me. So what I'd like to do, what we often carry, is a jackknife. A jackknife can be a many splendid thing. It may not be the most appropriate tool if you knew every-
10 thing you knew about what you were gonna have to use it for, but you can get by with it, and you can get the job done.

15 Statisticians like to develop procedures that are jackknives, in fact there's a whole class of procedures called the jackknife procedures, but what we do is, for those things that we can't verify, or those needs that we can't perceive, we have no knowledge of.

20 We want to have procedures that aren't going to be sensitive to those, so that what we say is appropriate under a broad spectrum of possible conditions that might exist, and the reason for going into that much depth on that particular point is that quite often, when you read an analysis of data, whether or not it relates to fibre measurements or not, you'll see certain assumptions going into the analysis of those data, and what we want to do is keep those assumptions that we can't verify, to a minimum, so that the statement that we make at the end, was appropriate, so we had that broad tool with us that allow our
25 statements to be correct.

30 Now one of the things that, the outcomes of the Round Robin study, revealed that the coefficient of variation were higher when we took the complete measurement of the inter-filter, inter-laboratory. In other words, one laboratory, the two filters were sampled on the same worker. One filter went to Laboratory A, one filter went to Laboratory B, and it was randomized. Then by comparing the measurements that Laboratory A

5 A. (Cont'd.) got on the left filter with the measurements that Laboratory B got on the right filter, we saw more variation as measured by the coefficient of variation. So that we can compare it with the previous thinking on the subject, the values were higher than had been previously thought. One of the reasons for a possible explanation for that is simply that we collected data in order to investigate the variability that simply had not been, that type of data, that extensiver data base had not been available before this point.

10 Now when we calculate, now here I have to apologize, I had some trans-, some of the tables that we have in the Round Robin study, the transparencies, are still sitting in Denver, so I can't put it up, but we arrived at, and here I'm referring to the tables that one finds in chapter seven

15 DR. DUPRE: I think Dr. Chase we all have copies, so if you want to simply refer us to the page number or the table number.

20 DR. CHASE: A. Well, going to the collection of tables at the end of, and graphs, the figure seven point three, those curves,

DR. DUPRE: We're not quite there yet.

MR. SAMPSON: I believe the figures are after the tables.

DR. DUPRE: Counsel, do you have it before you also?

MR. LASKIN: I think we've all caught up to it.

25 DR. CHASE: A. The final draft will have all of the page numbers on it, so one can find it a little more easily, hopefully, than this.

30 When I, and the thing I was talking about before, was the comparison between Laboratory A counting the left filter, and Laboratory B counting the right filter, that were simultaneously taken on a single worker.

That's the type of variation that's represented

5 A. (Cont'd.) in figure seven point three, the interlaboratory, interfilter variation, and when I say that though the variation that we found was greater than had previously been thought, it's those curves, those are coefficient of variation curves related to the number of fibres counted, which again is the way that people have been presenting those data, so it's we felt, it was appropriate because of the;

10 1: the way the historical presentation of the data, and

2: the number of fibres counted is an important consideration in terms of the uncertainty inherent, in the data.

15 So that those highest curves are higher than previously been thought, that the lower one is just that theoretical lower bound that no-one has ever thought, contended we would reach, but that has been put in just for a perspective, so when I say it's higher than previously thought, if we were to draw curves from previously thought, they would fall between those two curves.

20 So those represent then, the best estimates that we could arrive at, of the coefficient of variation, as we relate it to the number of fibres counted on the film.

25 DR. UFFEN: Q. Could I relate this if possible to a statement you made in your summary paper, where you were comparing coefficient of variations with other kinds of measurements other than asbestos?

DR. CHASE: A. Yes.

30 Q. So that, to get a grip on the size of things; it's on page sixteen of the measurement of asbestos levels in the workplace. Towards the bottom of the page, it says,

"To place the phase contrast method in perspective, a 1977 NIOSH survey of nearly two hundred approved monitoring methods, should CVs ranging from point 0 four, to point one three," which are very much

DR. UFFEN: (Cont'd.) smaller than we are looking at. It appeared to me to be smaller than your theoretical lower limit.

A. Oh, now those are not, the theoretical lower limit applies to asbestos, and not specifically.

Q. O.K., good.

A. And that's derived from the poisson distribution Dr. Rhodes mentioned earlier, and again, if I could, poisson is encountered, that term, well, I shouldn't say you'll encounter it a lot. If you look in the appropriate literature, you will encounter it a lot. If you stand on a highway, and count the number of cars that go by in a five minute period, as long as you're not standing next to a stoplight, if you go out in the middle, where things are allowed to sort of flow at random, if you count the number of cars that pass by in a five minute period, and then you do it in another five minute period, and then you do it another five minute period, the distribution that that number has, is a poisson, very close to it. If you count the number of radiation measurements during a specific period of time, you get the same kind of thing.

If I were to put a bunch of particles in suspension in this glass, and mix it up, and then I counted the number of particles in a small amount of that, and then I counted the number of particles in another same measurement, that would sort of fall as a poisson, and the analogous thing holds true with particles, fibres in the air as we think of them as being mixed up, then as they fall on the filter, as the air is being drawn through the filter, theoretically, we get a fibre as just as likely to fall over here on the filter, as it is over here on the filter, so we tend to get this, what's called the Uniform Distribution on the filter, and when you count, according to an unbiased counting rule, you will get theoretically then, a

A. (Cont'd.) poisson distribution of counts.

And that's where that lower line comes from.

5 DR. UFFEN: Q. And then the final step, am I right, is the theoretical poisson distribution for asbestos, is a lot worse than it is for similar types of measurements for other things?

A. Yeah, well the theoretical one ...

10 Q. Worse in the sense that the coefficient of variation is much higher.

A. That's true, although when you relate it to the NIOSH measurements, those are really based on empirical data.

15 In some instances, it might be theoretical, but mostly, it's empirical, and the study that Dr. Rhodes mentioned earlier, which was commissioned by NIOSH to investigate the same kind of thing really, we're investigating here except it wasn't with asbestos.

20 What they found generally was that they did tend to get coefficients of variation, and we'll see it when the report comes out, but they did get, when they took the collection step into account, and they did compare these things that were taken simultaneously, they tended to get values that were higher than were reported in that NIOSH bulletin, for the four contaminants of interest that they looked at.

25 The fact that we have found something higher than was previously thought by taking the collection step into account, is totally consistent with what they found. It's just that asbestos is more difficult to measure, so we have more uncertainty in the measurement.

30 DR. DUPRE: Q. Could you just give me a couple of for instances, of the substances that yield these lower CVs in the monitoring methods?

DR. RHODES: A. Any of the cadmium lead ...

DR. CHASE: A. Lead was the first one that comes

A. (Cont'd.) to mind.

DR. RHODES: A. They're all wet chemical methods, or mostly wet chemical methods.

DR. MUSTARD: Q. May I interject a comment here, or a question about this problem? It comes from Dr. Uffen's point. I've unfortunately been involved in particle counting for periods of nearly forty years, and the errors that you're talking about on the particle counting here, seem to me not very different from the errors which are well established in particle counting which I worked through in 1940.

Is there really any substantial difference in the logic of Berenson's assessment of errors and particle counting using optical microscopes for cells of the blood, and what you're actually looking at here?

Is this really an application in the industrial field for particle counting with the optical microscope, which really fulfills essentially the rules that he established?

Do you know what I'm referring to?

DR. CHASE: A. Yes.

DR. MUSTARD: Q. Is that approximately the same ...

DR. CHASE: A. You're in that same area, very much.

DR. MUSTARD: So it's really, Dr. Uffen, I don't think it's really substantially different than what's, people have to count particles to tell whether you've got red cells or white cells. That's always a built-in problem in the actual counting routine.

DR. UFFEN: The thing which I'm trying to grasp here is that, it seems to me that we have some of us perhaps for the first time, an indication of the best that can be achieved for asbestos measurements in the CVs for the theoretical poisson distribution, and whether we like it or not, it's worse than blood cells, or lead particles, or something else.

DR. MUSTARD: Oh, I believe maybe better than for

DR. MUSTARD: (Cont'd.) blood cells sir, don't be...
(REPORTER: Laughter.)

5 DR. CHASE: A. Yes, well, Dr. Rhodes was just phrasing another way of the thing, and I was trying to emphasize, and that was that in the chemical analyses for the other contaminants we're talking about, this poisson doesn't even enter into it. It's just that, the counting of particles gives rise to that discussion.

10 DR. MUSTARD: As a good example, if I might interject to this for Dr. Uffen, is if I try to estimate your amount of haemoglobin by counting the amount of red blood cells in your blood, I will have a high coefficient of variation because I'm caught with the particle counting problem, but if I simply
15 do a chemical estimate of the amount of haemoglobin in your blood, I'll have a very small coefficient of variation, and a much closer estimate.

DR. CHASE: A. That's an excellent analogy.

20 DR. RHODES: A. The theoretical lower limit for the fibre counting, or for particle counting is well, fairly well established. It's not a new idea at all.

DR. UFFEN: You hesitate?

25 DR. CHASE: A. No, I'm gonna regret saying this, but in fibre counting, the theoretical limit is actually a little bit lower than that, and using previous counting technics, that's the lower limit. Using the counting technique that was recommended in the article I referred to earlier, and I guess if
30 I'm referring to an article, I should give it to you just so that you know what I'm talking about, the one by Dr. Cooper Feldman and myself, the one that led to NIOSH changing the counting rules. It's somewhat analogous, what you do is count ends of fibres, and divide by two. It's sort of like to count a cattle herd you count the feet, and divide by four, but it's,

5 A. (Cont'd.) there are theoretical reasons why it's slightly better to do it that way, so that I'm not being faithful to my theoretical hat, if I don't just put that footnote in.

10 Now having said it, please forget I said it. So there are some, it's an interesting example where sometimes where one flopped. There was a theoretical lower bound. You can come at it from another direction, and find it, lo and behold, that wasn't the lower bound. It was just because you were looking at it with horse blinders on.

So please forget I, the silence indicates you've already forgotten about it.

15 DR. UFFEN: All this means is, I'm thinking about it.

20 DR. CHASE: A. Now, these coefficients of variation that are estimated from the Round Robin study, represent the uncertainty inherent in the count for a single filter, and quite often it's necessary or desirable, to take more than one filter, in order to assessthe, come up with an estimate of the time weighted average of exposure for a given worker.

25 So we now, these curves that are not direct, if we were to take, go into a workplace, and select a worker, and put one filter on that worker for the morning shift, or morning half of the workshift, and then put a second filter on for the second half, that involves two determinations, and it gets into the distinction between those different error categories that Dr. Rhodes had on the transparency earlier.

30 So in order to come up with an estimate on the uncertainty based on two fibre counts, that is two filters, it's necessary to now go through some manipulations in order to arrive at the uncertainty, for the final time weighted average estimate based on two filters, because these are for a single filter.

5 A. (Cont'd.) We have done that to illustrate the kinds of confidence intervals one might come up with, and illustrate, because we have in the discussion in the Round Robin report, all of the caveats that go into what distributions we use to illustrate this, are there, but if we could just cut through all of that, I think the technical caveats are in the report, but we can go to the table one on page unnumbered, which falls right after page twenty-five in the thing that was given
10 to you last night.

What we've done here is to calculate, to illustrate the sort of confidence interval that one might come up with, based on a measurement, and then the reason for going down here and saying the number of samples. That gets at the point that I was getting at earlier, and that is, to use those curves
15 for the coefficient^s of variation.

If I have a single filter to estimate the exposure, then I can use that curve somewhat directly to arrive at it. My confidence interval that's passing through all of those manipulations I alluded to earlier, that one must go through, in
20 order to use the coefficient of variation to arrive at the number.

However, when I get to more than one, filter, then I've got to go through some additional manipulations to arrive at that, but as Dr. Mustard indicated then the bottom line relates in the same scale, that fibres per ml, that we originally
25 wanted.

Now these refer to in example as an observed value of point one fibres per cc. Then the confidence interval that one arrives at in these examples, goes from , it goes roughly over an order of magnitude there, from something, point 0 two to point three roughly, and the fact that we've taken these
30 off to the second and third decimal point does not, please don't interpret that as being that we can measure to the second and

5 A. (Cont'd.) third decimal. It's simply necessary to do that, in order to make these calculations for the illustrations.

Here we're going down to a measured value of point five, one might come up with a confidence interval that would range from down in the range of point one up to the range of one.

10 Likewise then, the example some examples that we have used for an observed value of one.

MR. SAMPSON: Q. Dr. Chase, can I ask you a couple of questions about this, to maybe try to explore it a little bit.

15 Let's take the example of the middle row there, the TWA measurement of point five fibres per cc. Now that would be a case, and let's take the top line there, which is one eight hour sample. Now what that would mean is that if an employer went in, and took one eight hour sample of a worker on day - today, and got a measurement of point five, those ninety-five percent confidence limits on the right hand column there, indicate, are
20 an illustration of the range of likely true values that that measurement might reflect with ninety-five percent confidence.

DR. CHASE: A. That's correct.

25 Q. Now let's just hypothetically say that the recording angel knows that the real value was on the high end of that range, so that the employer measured point five, but the actual exposure was one point three seven.

30 Given the variability that's shown generally here, on this table, if somebody else had come in and measured that same worker on that same day, using again, a one eight hour sampling protocol, how high a measurement conceivably could that separate independent sampler have gotten, just in general terms? I recognize the table here is ...

5 DR. CHASE: A. It could be in the neighbourhood of two, and the reason for that is, I come in, and measure, and I arrive at a confidence interval that goes say from point one three, to one point three seven, taking that example.

10 And suppose that this is one of those ninety-five percent times that I'm right. My interval contains the, quote, true value. Then it sits right up here at the high end of the range. But I'm right, my interval contains it. But then let's take that hypothetical situation where somebody else had been there that same day to measure that same worker, and they got a value that sits up here at one point nine. That was their measured value. I measured recall down here at point five. Let's say they're at one point nine. They construct a confidence interval that goes, and I'm pulling this one from my head, from 15 one point two, to two point five, and we said that the value was right in here. I should move the one point two down a little bit.

20 So they're right also. On their tally sheet, they've made a correct calculation of the confidence interval, and that's totally compatible with the kind, and I'm taking this is one of those extreme examples of what can happen, but quite likely in varying degrees.

Q. Is it a probability of greater than five percent that something like that could happen?

25 A. Well, we would both be right, would happen point nine five squared. That we would both be right on a given day, if we're both constructing ninety-five percent confidence intervals, will happen about ninety percent of the time, because, now you're not asking that one person be right, you're asking that the second person be right also, and we're getting into some probabilistic arguments when you talk about that.

30 So the chance of having both people correct, will happen about ninety percent of the time, if they're

A. (Cont'd.) constructing ninety-five percent confidence intervals.

5 Q. Does that mean that's a ten percent chance of their being wrong?

A. No, there's a ten percent chance that they both won't be right.

Q. No further questions.

10 A. Let me just, this thing is drying out; if I set it upside down, will it regenerate itself? (REPORTER: this remark has reference to the chalk.)

15 This was Industrial Hygienist One, and this is Industrial Hygienist number Two. Number one could either be correct, or incorrect. Number two could either be correct or incorrect. And all four of those, the thing that we were talking about was that the chance of them both being correct, is point nine five squared.

20 The chance of this one being correct, and this one being incorrect, is point nine five times point 0 five, and here, it's point nine five times point 0 five, and the chance of both of them being wrong, is point 0 five squared.

When you begin to talk about the chances of two of them happening, you have to talk about this four fold table, and when I said about ten percent, it's these three things combined.

25 Q. But when Industrial Hygienist number Two is the enforcement officer, then Industrial Hygienist number One has to really consider all three of those blocks outside of the situation except when they're both correct.

A. That's right, that's right.

30 Q. So if point nine five squared is about point nine, then he's got to consider a ten percent possibility that the Industrial Hygienist number Two will come in and find a different measurement, and in effect, claim a violation, based on that

Q. (Cont'd.) measurement.

5 A. That's correct. You're getting, those are the kinds of consideration that need to be taken into account when the, if you will, the company industrial hygienist takes the measurement on the worker, and would like to infer something about what might happen if someone else were to do it.

10 Q. O.K., well going back to that example we were talking about a minute ago, that one eight hour measurement, and you come out with point five, this table one here on the screen suggests, correct me if I'm wrong, which there's a ninety-five percent possibility of that, that suggests that the employer, or the industrial hygienist who took that measurement can only be
15 ninety-five percent certain that the worker he measured on that day, was exposed at a level of one point three seven fibres per cc, or less.

20 A. Yeah, well actually, it's ninety-five percent certain that it would fall between the point one three, and the one point three seven, because we constructed our confidence interval to allow two and a half percent on the high side, and two and a half percent on the low side.

Therefore if we're gonna say, how confident is he that it's less than one point three seven, we have to say ninety-seven and a half percent.

Q. O.K.

25 A. But it is an important point in terms of if you were to construct a confidence interval in terms of two sided or one sided. It's possible to do it both ways.

Q. But this illustrates the two sides.

30 A. This illustrates the two sides. But in some of the calculations that I'm going to refer to later, they were one sided, which is the most appropriate way in the context in which we'll get into.

5 DR. MUSTARD: Q. May I ask a question about the data in table eight - one, in the exhibit that was given to us tab one of the exhibit, or is it tab three? It's tab three of the exhibit, and table one of the material, what's it called, tab one, the one that we just got yesterday?

MISS KAHN: Tab eleven.

10 DR. MUSTARD: Q. Tab eleven. The data in table one is derived from the data in table eight. I'd like to make certain I've got all of this clear.

DR. DUPRE: Do you have table eight - one?

DR. CHASE: Yes, yes.

DR. DUPRE: Just before we get into this, does everybody have table eight - one, out of a Round Robin study?

15 MR. SAMPSON: Table A - one is at the end of section eight - three, if anyone is having difficulty finding it. Well, they're written on there, and they didn't come through on the Xerox, unfortunately.

DR. DUPRE: Are we all right? Then please proceed Dr. Mustard.

20 DR. MUSTARD: Q. Just have this first point, these are real data. They're collected from a Round Robin study. Is that correct?

DR. CHASE: A. No, no, those are ...

Q. These are theoretical data.

25 A. That's right. It was said, suppose we got a value of this.

Q. Well then, let me ask a question, on as I recall vaguely some of the calculations that go into particle counting, the actual number of particles counted is an extremely important determinate of the error estimate.

30 A. That's correct.

Q. On table eight one you have the what I presume

DR. MUSTARD: Q. (Cont'd.) relates to experience, that the higher the fibre concentration, the more fibres you'll count in the field. Is that correct; is that what that estimate is in table eight one, is supposed to show?

A. That's correct.

Q. What happens to your calculations if you go the other way, and say that you will require the same number of fibres to be counted. In other words, you go to a number of fibres that must be counted, then estimate whatever you say, I'm going to have to count seventy, right, let me take the TWA of zero point one fibres per cc., which you got, seven fibres counted per filter, and now if you go down to one fibre per cc, we're counting seventy fibres per filter. But you told me you take a segment of the filter to count, so that you've got more of the filter there.

You could therefore, technically particularly using scanning systems, automatic counting, force onto the system a requirement that they all count seventy fibres, have to go to seventy fibres.

Do you have any idea what that does to your confidence limits estimate?

A. Yeah, I can give you some ballpark, that's an extremely good question, and one that people have said we can solve the whole problem by simply, when, if the biggest source of air is from our counting procedure, then we can eliminate a lot of the problem, by counting more fibres, and in fact, well, let me just digress for one second. The counting procedure for the rules that are generally followed, the ones that, the NIOSH rules that were followed here, call for counting at least, you first begin by counting twenty fields of view. Then, if you've already reached one hundred fibres, you can stop, but if you haven't reached one hundred fibres, you continue to count until one of two things happen. Either you've reached one hundred fibres, or you've

5 A. (Cont'd.) reached one hundred fields. And the thing that you're suggesting, would say, let's require everybody to count, we could change those rules, to make sure that they counted that one hundred fibres more often, and don't allow them to stop at one hundred fields, but let's extend that out and count more fields.

10 It would predictably improve the error component from that uncertainty of the distribution on the filter.

15 The available data suggest that it would not have a dramatic impact at all on the total uncertainty that we had. And I rely partially for that statement on some theoretical calculations that I made at the time of the initial thing, where the bias was uncovered in the counting rule, but I don't have that to give you.

20 DR. MUSTARD: Q. I guess my reason for the question is, having to count myself in these fields, the requirement to reduce the error component in the counting part of the system, was that you had to go up almost exponentially in the number of particles you counted that have significant effects on the error.

25 But I guess the reason for my question is, with modern electronic counting methods, which are certainly well established in other areas of particle counting, and we've had some evidence that they may be, are being applied in this field, do you have any evidence of, when you now have that electronic capability, to increase your particle counting significantly, you know, by a factor of tenfold?

30 What does that do to the confidence estimates, do you know? I mean that's a big change in the particle counting. I realise the numbers of particles ...

A. Yeah, there I wouldn't really have a feeling for it. Within the context of counting having the microscopist count more, there I have a feeling for it.

5 A. (Cont'd.) If we go into another arena, where something else is doing the counting, then what I would like, is a Round Robin of those automatic techniques.

DR. MUSTARD: Q. But what you could do from established data in other fields of particle counting, where there have been theoretical estimates, and actual estimates worked out, about increasing the number of counts done.

10 You could plug that into your model here, and just see what it does sometime, couldn't you.

15 A. That one could do, yes. I would just add the other caveat that some of the particle counting, the automatic particle counting techniques in the fibre thing, requires some technical break-throughs, before that would be, but I know you're aware of that.

Yes, but certainly one could go through those kinds of calculations.

20 DR. RHODES: A. Gerry, could I comment? The thing that you're working on there though, is the sample evaluation step, and it would not impact on the collection ...

DR. MUSTARD: I realise that, and I'm trying to see what the magnitude of the respective contributions to this are, really.

25 DR. UFFEN: Q. Am I right, this shows up in this table eight one, and it doesn't show up in table one?

DR. RHODES: A. That's right.

Q. There's an appearance of a better coefficient of variation, but when you look, you find you only measured seven fibres, so it may be a, is this right?

A. You've got seven fibres per filter.

Q. Per filter, well, yes.

DR. CHASE: A. But eight samples.

30 DR. RHODES: You've got eight samples. What we did

5 A. (Cont'd.) on that, by way of explanation, is just take the total number of fibres we had to count in an eight hour period, to give that concentration, and then assume that they would get equal numbers on each filter, rather than trying to play games, randomizing the numbers on the filters.

It was an illustration, rather than a firm example of exactly what you would expect.

10 DR. MUSTARD: Q. The other question that I have in relation to those tables is, I assume that you have calculated, well you indicated that you'd calculated the ninety-five percent confidence limits by using the logarithms of the data. I presume that's what you've done, that as the actual calculation that you've transformed them back into your numerical units. Is that correct?

15 DR. CHASE: A. That's correct.

Q. Is the coefficient of variation calculated using the logarithms of the data as well, or is it meaning that these are analogous calculations are all done at the same transformation of the data?

20 A. No, and it has to do with the coefficient of variation is a quantification of the, of that uncertainty that's inherent in the measurements we're getting. And that's related to the actual measurement.

25 Now when we talk about using the logarithms, it was, we used the logarithms because we were taking a log normal distribution, and when we have a log normal distribution, we get that, if this is zero here, we get something that tends to come up, and then gets spewed out, and this is the actual count, whereas, if we were to take the log of these numbers here, and plot the same thing, we'd tend to get, so here, this scale down here is the log of this scale up here. It stretches ...

30 Q. I know that. Now I have a problem. That means

DR. MUSTARD: Q. (Cont'd.) that the zero point one fibre per cc which is a mean, is not a geometric mean.

A. That's correct.

Q. That's an arithmetic mean.

A. Yeah, and ...

Q. O.K., now it would be awfully useful for me, if you could give me the geometric mean, and the confidence limits, as logs. Because you know, now I have a problem of situating, and in my field, I'm trained if I do have transformation of the data, I have to give the geometric mean, and my confidence limits or estimates, using the logarithms of the data as well.

Now I'm very uncomfortable, because I have a mean I can't relate to your confidence limits easily.

A. O.K., well, we could get it by taking the logs, and then just taking the mid point, 'cos it sits in the middle of the logs. And if you are already familiar with a log normal, then you've probably seen where we get, let me start off with something that I'm measuring. Let's say that X is my fibre per cc or ml, I'm jumping back and forth, that's my measurement. And if I take the log of X , we're saying that that has something like a log normal.

If the log of that has this mean of μ and σ squared that we referred to before, then X has the mean E to the μ plus one half σ squared, and the geometric mean is relating to μ , and the arithmetic mean is relating to either the μ plus one half σ squared.

So if we were to take, when we construct the confidence interval, we're dealing with the log of X , so to get the thing that you're referring to, we have to take the logs of the two end points, and then the geometric means is gonna sit in the middle of it.

Q. O.K., so that means that that zero point one

5 DR. MUSTARD: Q. (Cont'd.) fibre estimate, expressed as a log, calculated in the logarithm, and transformed back, would be higher than zero point one. It would be somewhere I would presume, am I right or wrong in that estimate?

A. No, the measurement of point one would say that the mid point of that distribution ...

Q. Let me ask a simple question.

A. O.K.

10 Q. Can you tell me what the geometric mean, transformed back into regular units would be in that calculation?

A. Yeah, it would be less than point one.

Q. O.K., thank you.

15 A. In fact, that's an algebraic identity. The geometric mean will always be less than or equal to the arithmetic.

20 Q. So that I would be fair to say then in looking at that, that I'm really looking at, if I'm going to be consistent, at least for me from my background, that let us say that the geometric mean, transformed back into arithmetic units, would be zero point eight fibres.

Do you follow me, on the top line?

A. Yeah, it would be point O eight.

25 Q. Point O eight, so if I'm really talking about the confidence for me, I'm talking about point O eight with those confidence limits, rather than point one.

A. O.K., yes

Q. For me, that's easier.

A. Yes, Exactly.

30 DR. MUSTARD: Sorry, that's all. My confusion has been cleared.

DR. CHASE: Is it legitimate to suggest a break?

MR. SAMPSON: Yeah, it's been suggested by me.

DR. DUPRE: Shall we rise for about fifteen minutes?

THE INQUIRY RECESSED

THE INQUIRY RESUMED

DR. DUPRE: You may proceed, counsel.

MR. SAMPSON: Dr. Chase, do you want to continue with your summary of your statement?

DR. CHASE: O.K., is this all right my talking from standing? Are you picking it up O.K.? I feel much more comfortable rather than sitting down here in the corner, if that's all right. Also nine years in the classroom, I feel kind of ...

We mentioned before, the situation for a given day, what we can interpret, how we, what kind of an interpretation, what sort of uncertainty we can attach to the measurement that we achieve, that we as one individual. The fact that that doesn't tell us exactly what another individual might have achieved on that same day.

Well, for a given day, there is if will, a true value. That's the one we're trying for. That's the thing we're estimating, and that's the thing that only the recording angel knows, that we're trying to estimate it. But in terms of the total picture, it's necessary to bring in an additional variation, and that is the fact that the true value today, will be different, predictably different than the true value yesterday, and Monday, it will be different than it was today. That from one day to the next, the workplace will vary in its true average airborne concentration, as we average over that period.

This is widely accepted by I would say anyone who has worked in the area of looking at these numbers, and the

5 A. (Cont'd.) distribution, all the available evidence tends to suggest that as we plot one day, and then the next day, and then the following day, and we get this spectrum of values for true values from day to day.

10 We'll get something that tends to be skewed, and very many authors have suggested that it looks very much like a log normal, and the log normal comes in because it's a convenient one to work with. It gets at this idea that things can't go below zero. We know we don't have negative fibre concentrations, but we can get an occasional pi value for an average, so if we were to plot the true means, and of course again, these are the only things that the recording angel knows, we might get something that comes up and then trails off on the high side, which is the classic log normal plot.

15 So if you will, a value is selected for, at this point right here, for August 14th, and then on August 16th, a value happens to come up down here, and on the 17th, it might be out here, and as we go through the days and months, we're going to get a collection of values of those true values.

20 Now of course, when we're on August 14th, once the recording angel has selected that one, for August 14th, then we come in and try to measure it, we might get a value that's below the one for August 14th, or we might get one that's above it. And the reason that we vary about that particular value for that day, is because of our measuring technique.

25 That brings us into the type of area we've been talking about in terms of the measurement for a single day.

Now there are two things that one, two objectives in the measurement and the estimation of the worker exposure.

30 One of course, is one wants to be reasonably confident that you're not going to be found out of compliance, if someone were to come in and measure for compliance.

5 A. (Cont'd.) And secondly, and more importantly, you do not want the worker to be exposed above the permissible exposure limit.

10 So when we talk about the measurement, what we use the measurement for in a single day, we want to infer something in the total picture of compliance, non compliance, and protection of the worker in terms of not only that day, but the days you don't measure.

15 If we measure, the day we happen to measure is a sample day from these values that come up, so we've got uncertainty not only in the measurement we take on a given day, but we've got the uncertainty of - was that day a typical day, or was that day a non-typical day? And I've put typical in quotes, because what we've got is a spectrum of actual day exposures for that worker.

20 Now I mentioned that the idea of day to day variation is accepted. There's general agreement on that, and furthermore, this idea of describing day to day variation, and saying it can be approximated somewhat by a log normal distribution, is also, seems to be pretty general agreement, that at least for illustrating things, it's the distribution that's quite appropriate.

25 Now we've said that you can describe the spread of a distribution by the coefficient of variation. You can also describe the spread of a distribution by the geometric standard deviation, and where we were talking before about the log of X having something that has a mean, and a variance, and standard deviation, that the geometric standard deviation is just sigma raised to the E power; I mean E raised to the sigma power, pardon me.

30 So in other words, it's necessary to get into this discussion, to use the term both coefficient of variation to

5 A. (Cont'd.) describe the uncertainty in the measurement of the asbestos level on a given day, and the literature describes these day to day variations, by referring to geometric standard deviations.

10 There is a one to one relationship between a geometric standard deviation, and a coefficient of variation. It's just that in order to relate to the literature, day to day variation is generally described by saying, the geometric standard deviation is thus and so, and the uncertainty inherent in air contaminant measurements is generally described by coefficients of variation, but we could draw up two columns, and say, there's a one to one correspondence.

15 So, in order to combine those two things, it's necessary to take into account both the day to day variation, and the inherent uncertainty on a given day. So we've got those two curves if you will superimposed, because on a given, here is the here's this curve that I've used to describe day to day variation of true values, and if on a given day, that happens to be the value that we're at, then down here, we'll have a distribution of values that we might get about that particular measurement on a given day.

20 So up here, it's day to day, and down here, it's on a given day.

25 So the first source of error that we have is, how much uncertainty can we say about what the actual level was today, then having quantified that uncertainty, we want to take into account this day to day variation to get at this concept of being reasonably sure that one is not going to be found out of compliance, or being reasonably certain that that worker will not be exposed beyond the permissible exposure limit, more than a small fraction of the days.

30 This gets at the same type of concept that NIOSH

5 A. (Cont'd.) got into when they were discussing action levels. The technical derivation, the actual things we're talking about here, are exactly that kind of thing.

10 In other words, if going back to some of the examples we had before, when you take a measurement, when we say that we'd like to come up with some range that we're reasonably confident that it falls within that range, well, once we get to this, the concept we're dealing with now, it brings up that problem of not two sided intervals, but one sided confidence intervals, because if it's very very low, I'm not interested in distinguishing between the very very lows. What I'm trying to do is make sure that it's no higher than some value.

15 So the thing I'm getting at now, is not using two sided confidence intervals, but one sided.

DR. UFFEN: Q. Would you mind a small interruption here?

20 I follow this all right, and I see the significance of it, but there's a little statement in your paper related to this which say that

"The interday variability in actual concentrations cannot be controlled."

Could you explain to me why it cannot be controlled?

25 A. Well, that statement is made from the point of view that there are inherent things that vary from day to day, that simply that the random nature of dust clouds arising from activities, that will vary from day to day.

30 Certainly there are certain, I guess what we're doing is, that it gets almost to that systematic air versus the random air in those things that contribute to the dust cloud in the workplace, and we're talking about those things, that inherent variability in the dust cloud in the workplace that's

A. (Cont'd.) going to be there regardless of what you do.

5 DR. UFFEN: Q. The random part of it is what you're talking about. If there are any systematics, that's excluded from this analysis, and they presumably might be controllable.

10 A. Yes, one would hope that the systematic things that - a given operation with the similar, you know, the dust control equipment is operating, the work practices have not changed, the product reasonable consistent in terms of the raw materials going into it, but that combination of factors of how the ingredients vary, and the way things go from one day to the next, you'll see a variation.

15 Just like I guess, going to measurement of various things on our bodies, that from one day to the next, will simply not be the same, even though it's the same body.

20 So in order to, the bottom line that happens here then, is in order to be reasonably certain that you're not exceeding a point, which would be the permissible exposure limit, the standard, it's necessary to reliably measure well below it, 'cos of course, from the inherent uncertainty, if the permissible exposure limit was one half, and if you measured one half, we know from the errors that are inherent, that that certainly gives us no assurance that someone else couldn't come in and measure well above it, or that the actual measurement, the true value, wasn't something above it. It might have been below, it might have been above, it might have been right on.

25 So in order to be, in order to reasonably assure oneself that you're not exceeding a given point, it's necessary to measure well below it, and as I mentioned before, this is, the concept is identical to that that was discussed by NIOSH in
30 their discussion of action levels.

5 A. (Cont'd.) And when one does that, in order to make the calculation, so first of all, I have to, I think I garbaged up another one. (REPORTER: Reference to chalker.) I have to reliably measure the low, the permissible exposure limit, in order to give myself this assurance.

10 And what I'd like, in the example, the calculation referred to in the submission, and the example, I'll show you, is based on at least a ninety-five percent confidence. So we get into that confidence interval concept, that no more than five percent of the true exposures would exceed the permissible exposure limit, and in order to discuss the thing we're talking about, it's necessary to bring in two probabilities.

15 You need the confidence level, and the permissible, the percent exceeding the permissible exposure limit. And that five percent, and ninety-five percent again, have been taken from the NIOSH, for purposes of the illustration.

20 Right, then, one other assumption, or thing to work with here, and that is there's general agreement, it's pretty much a consensus that below a tenth of a fibre, you have difficulty in distinguishing between values.

It's difficult to measure below a tenth. We cannot, and that's reliably measure, or another way of saying that is, it's just extremely difficult to distinguish between values that fall below a tenth of a fibre per cc.

25 Those are the two things that I then use, to arrive at what we would consider a lower bound, below which you couldn't even begin to do the kind of thing that we're talking about.

30 So we'd like at least to have the ability to be ninety-five percent confident that no more than five percent of the values would exceed the permissible exposure limit.

Taking then between day variation, the uncertainty

5 A. (Cont'd.) in the measurement itself, and this assumption right here, that gives us lower bounds below which, we can't even do that, and now the column heading for the geometric standard deviation,

DR. DUPRE: May I just ask what the source of this is? Are we going to label in other words, Mr. Sampson, are we going to label this?

10 MR. SAMPSON: I think we'd like to. We haven't submitted this. This was as you can see, just recently done.

DR. CHASE: It should have been a table at the back of what we've ...

MR. SAMPSON: Can we call it Table two, and we'll submit extra copies of it as soon as we can.

15 So this would be table two to the written statement to the written statement that was submitted.

EXHIBIT NO.,39 TABLE 2: The abovementioned document was submitted at this point.

20 DR. CHASE: A. Across the top here, I'm taking the geometric standard deviation. Now that is a measure of this day to day variation. As the geometric standard deviation increases, that says that there's more variation in the workplace.

25 These particular values were selected to bracket the mid one point six range, which was stated by NIOSH, but pretty much described the middle ground of day to day workplace variation.

30 The row headings are the CV values, and they're the range, for example if you go to table one that we have, we find that the lowest value in there is somewhere's in the neighbourhood of point four, and it could range on up.

It could range from roughly point four up to point

A. (Cont'd.) seven.

5 So we're trying to pick the GSDs from the NIOSH considerations, the coefficients of variation from the results of the study.

10 Then, going through the calculations that I alluded to that would give you this absolute lower bound, that would even begin to permit a calculation that would give you this assurance value. That would say that, for example, if you had a GSD between day variation were described by geometric standard deviation of one point five, and you were measuring the combination of things that you measured on that day was such that the coefficient of variation for the measurement that you had were point four, you could not even make the calculation, if the standard were point four nine. So it's roughly point five.

15 And the reason for that is that in order to make a ninety-five percent calculation, a calculation which with ninety-five percent confidence, would say that no more than five percent of the values would be above point four nine, it would require us to distinguish between values that fall in this less than a tenth of a fibre per cc range.

20 So as we increase of course the uncertainty in the calculation we make, as we increase the inherent variation in the day to day, that pushes up, whether we go down or over, you see, there's an increase that pushes up this lower bound for the permissible exposure limit, and I would emphasize lower bound, because this is just the point at which you can even begin to make the calculation.

25 When one gets into this discussion, I think it's appropriate to bring in the perspective that if one has a standard that's set say at a value of one, in order to be in reasonable compliance with that, in order to assure oneself, there are practical limitations that force the measurement to be

5 A. (Cont'd.) taken. You have to measure well below it to give this assurance, so that the nature of that would force from the statistical considerations, force the measurements to be well below the standard, and the practical limitations are such that it's the same thing that happens.

10 In other words, on the outside of a factory, there isn't a big dial that one can turn to two, or one or whatever. Within that factory, there's a spectrum of exposures, and what we're saying is that that spectrum of exposures, in order to be in compliance, one has to have the worst, the highest levels in control, and in order to have the highest levels in control, those values have to over time, be well below, the spectrum of those exposures have to be well below that standard value.

15 DR. MUSTARD: Q. Can I interrupt you at this moment?

DR. CHASE: A. Sure.

Q. The material in the body of that table, is a theoretical fibres per cc estimation. Is that correct?

A. That's correct.

20 Q. And all this is theoretical really. It's the mathematical application of material that was in the previous information we were looking at.

A. That's correct.

25 Q. And this is the upper limit of the ninety-five percent confidence limits. Is that right, that thing shown on the table here?

A. Well, yeah.

30 Q. In other words, if I looked at this, and took your lower right hand figure, I could say that given the day to day variation shown on the top, and given the coefficient of variation with daily counts shown on the lower left, and if I set a fibre limit of one fibre per cc approximately, I'd be

DR. MUSTARD: Q. (Cont'd.) ninety-five percent confident that that, as I set that as my upper limit, that
5 ninety-five percent of my results would actually be below that, for time immemorial.

Is that what I'm being told?

A. Yeah, I'll need to have one more. You're almost there. A measurement down in the range given these highest
10 values that are on the table, a measurement down in the range of point one, would allow me to make the statement that I can be ninety-five percent confident that no more than five percent of the values will exceed one.

Q. That's right. But we also have some problems in terms of the limit, the technical limit of the estimate
15 problem, when you get down to low fibres as well.

A. Yeah, and that's where I'm getting the one value, from the technical limit of the difficulty in measuring
down here.

What this table says basically is, I've taken a condition that would be given as ability to reasonably assure
20 oneself that no more than a certain percentage of days exceed a given value. As a necessary condition for setting a standard, not a sufficient condition, but a necessary one, and that's why I emphasize the lower bound.

But yes, it is a theoretical calculation using the values from the NIOSH publication, from our study here, and
25 taking the log normals as the distribution to illustrate what happens with that lower bound.

Q. Right now, let me comment on one other interpretation from that data, which would help me in terms of my
own clarification. If I now go up to the upper left hand part of the main body of the table, where you have the figure point four
30 nine, if I selected then a fibre controlled level of the upper

5 DR. MUSTARD: Q. (Cont'd.) limit of point five, it would mean that when I had GSDs of one point eight, and CVs of zero point seven, even though I had an estimate that controlled them but at point five, that when I was measuring a level of point one, and had those variances really showing up, that I really couldn't be a hundred percent confident as at point five, it could really be up as high as one point O nine.

A. Yeah.

10 Q. However, the real difference would be in putting all that you said together, is that if I did adopt the level of point five versus one one, I in effect would be driving the day to day fibre levels much lower with the point five, because of the, in other words, I would be in effect I'm somewhere down around point two or point one.

15 A. Yeah, the one thing that we say, we're doing that. It's just that we wouldn't have the ability to reliably assure ourselves that that was happening.

20 Q. Yes, but if I could measure the point five, the probability would be that I'd be likely to be down there rather than above point five.

A. Yeah, that gets into a qualitative rather than a quantitative.

Q. That's right, but the probability would be in effect, that would be controlled. Thank you.

25 A. Yeah, well, the point one that is selected here was chosen because I think there's a consensus of the point one.

In some situations, that might be low, and in other situations, it might be a little high, but it's simply a consensus value that was selected in order to illustrate.

30 So, you know, the bottom line here is then that what this implies is that you have to get up into that, the

5 A. (Cont'd. ranges above these values, if these are the actual situations, that would give the employers an adequate range for monitoring, and then another corollary to that then, is that that provides the adequate highly protective thing in terms of pushing the values well below that in terms of most exposures, and the worst case exposures.

10 MR. SAMPSON: Dr. Chase, being a hardy soul, I'm going to try and ask a question, and see if I can summarize this.

15 Just taking the two highest values for the GSD and the CV on that table there, would I be correct in saying that if the inner day variability were accurately described by GSD of one point eight, and the measurement variability on that day was accurately quantified as point seven, that in order to be ninety-five percent confident that the worker I'm sampling isn't exposed more than five percent of the time at the level of one point O nine, I have to measure that worker at point one.

DR. CHASE: A. That's correct.

20 Q. And a similar conclusion would be true for each one of the values in the body of that table, i.e., that in order to get the requisite degree of confidence that the levels in the body of that table are being achieved, you have to be measuring the worker at a level of point one, and if you measured that worker at a level higher than point one, you don't have ninety-five percent confidence that he is not over-exposed more than five percent of the time.

25 A. That's right. The corollary to that, just to extend it a little further, then in your first thing, when you took the one point eight, and the point seven, that would, the corollary to this number would say that this would imply that this and this were the real life situations in most situations. Then the standard should really be set well above this to give

30

A. (Cont'd.) you a range in which to be able to reliably assure oneself of the thing that we're talking about.

5 So in each case, this number says that the standard should be well above that, because that then gives you a range in which to measure to get that assurance.

DR. MUSTARD: But there's the opposite was to express your statement I believe.

MR. SAMPSON: Yes.

10 DR. MUSTARD: That is to establish that the upper limit shall never exceed one, which means you have a ninety-five percent probability of everything being below that.

15 Do you follow what I'm saying? In other words, if you said, the upper limit, not what the average should be, but the upper limit to what the exposure shall be, shall be one, then in effect, you'd be controlling at point one, but that's just the reverse to what you're saying.

I expressed it the other way around.

MR. SAMPSON: I think I'll defer to the witness on that. I'm not sure.

20 DR. CHASE: No, that is a way of expressing it.

DR. UFFEN: Q. Are you going to proceed to talk about the problem of the systematic errors, or am I getting ahead of you?

25 A. No, this, really, what we've done here, is by taking the CVs that were derived from the bottom line, the between-filter, between-laboratory, all the errors that we're able to measure are inherent in this, so this is the , the CV curves that we've used to go through these calculations, are implicit in all of these calculations, so all the inherent errors that were there, have been..

30 Q. I maybe am not making myself clear on it. In your Measurement of Asbestos Level paper, on page 22, after you've

DR. UFFEN: Q. (Cont'd.) dealt with this situation, you then proceed to talk about the implication to standard setting, and the difficulty of incorporating systematic or persistent errors.

MR. SAMPSON: I think that's in sample collection, correct?

DR. CHASE: Oh, O.K.

DR. UFFEN: Q. Are you going to, maybe I got ahead of you.

DR. CHASE: A. Well no, I guess that has to be left just as a footnote in a sense, because it's an error that we were not able to measure using this. Therefore that would have the effect, if we, had we been able to quantify it, which we weren't it would have the effect of driving up those numbers, because it's contributing more to the variation. But we simply do not have a handle on what the magnitude of that variation is, other than a conjecture that that would, we would not expect it to dramatically increase them, but it would predictably have in it.

Q. Am I right that anyone who is concerned with standard setting, would have to include this, or it would be leaving out an important aspect, which requires the measurements to be much lower than the standard you are trying to achieve, an additional one.

A. Yeah.

Q. And I gather that it's, as far as you're able to determine, it's not known, the Round Robin study doesn't give you the information necessary.

A. Yeah, it would just be another thing that would emphasize the lower bound, the nature of the numbers here.

Q. In a lot of this type of work, where we don't have much choice, people will say, Well, we'll divide by two, or we'll multiply by two, or something like this, you know. You'd make

5 DR, UFFEN: Q. (Cont'd.) guesses. Would you be in any position, able to say to us, if you were trying to achieve a regulated level of one fibre per cc, what allowance would have to be included for systematic or persistent errors?

A. I think I would - would you mind saying that one more time?

10 Q. Having achieved an analysis that allows you to take into account the random errors, you have a statement on page 22,

15 "Using this method, the lowest asbestos standard which permits the possibility of achieving a desired degree of confidence, without requiring measurements below zero point one fibres per cc, falls in the range of zero point five, to one fibres per cc." Then it goes on in the next paragraph,

20 "The estimated ranges standards is intended merely to illustrate the difficulties. Our estimates do not consider the additional variability which might be present due to systematic or persistent error in sample collection, which we could not measure in the Round Robin study." Now from the point of view of a person who might be required with establishing a sensible regulation, what do we do?

25 A. O.K., I guess there's two parts to my answer, the first one being how to treat the unknown, the thing we weren't able to quantify. I would not regard that as a major problem, but certainly something that would be need to be aware of.

30 The second thing is that one would have to go through a series of calculations that haven't done considering workplace environments that would give a spectrum of numbers that

5 A. (Cont'd.) would give people a reasonable chance then, of achieving the kind of thing that we're talking about. So that would like I said, you say a factor of two for example. That might be quite a reasonable number to do, but I would, the nature of my beast is such that I would try to quantify it, by saying well, this would you know, that would allow people with most of the time with this type of a hypothetical workplace day to day variation sort of thing, to achieve this assurance
10 that we're talking about.

So I have to add another probability, or another proportion, to say that in, you know, most of the time now, one would be able to achieve this by maintaining levels in this range in which we can measure, bringing into this factor here.

15 And perhaps a factor of two would be the one that one would arrive at. At least we could begin to, in the same way that we arrive at these numbers, by taking examples and distributions, one can take some additional, add one more level of calculation in here, and arrive at a further characterization of various standards, according to various criteria, and then
20 that would do it.

I'm being very general in saying this.

Q. Has anyone attempted to do that to your knowledge?

25 A. No, not precisely as you've asked it. It would be a spinoff of the NIOSH action level type of thing, but it's adding additional considerations to it.

Within constraints, I could do, I'm saying what I would do, so I guess I could do it, and I can offer to do that.

Q. Not on the board right now.

A. No, my calculator battery would run down.

30 MR. SAMPSON: Q. I was just going to, in an effort to try to put some quantitative handle on that, what if the CVs

Q. (Cont'd.) on the left hand column there, each went up by an increment of point one? If you were to, if we
5 knew what the persistent and systematic error in the sample collection step was, and the result was that those CVs each went up by point one, do you have any idea how the numbers in the body of that table would change, or whether or not they would change substantially?

10 A. Well, I mean, you can just drop down a notch, and we'd have to add another row.

Q. Well, what would the bottom row look like, or would it be a ...?

15 A. As you look at it here, and this will give you a ballpark figure for it. As you go from, here you jump up point O five here, point O six here, point O seven, a ballpark figure would be point O eight.

Likewise, here, seven, eight, ten, O.K., ten or eleven, point one , point one to one (Ph) would give you the ballpark answer to what you're saying.

20 Q. So the range of figure, the range of lower bounds that that table now represents, which runs from about point five to one point O, might shift up slightly, if under the hypothesis that I suggested, which was that dialling in the impact of the persistent and systematic error in the sample collection step, resulted in a point one increase in the coefficients of
25 variation.

A. Right. I think it's probably appropriate to mention that I referred to the NIOSH calculation of the action level quite a number of times.

30 The characterization of that that I've used here, is slightly different than their's. If I had used their formula, those lower bounds would be somewhat higher, and it's sort of a technical reason, and I don't think, there's no reason to get into.

5 A. (Cont'd.) It's just that the point four nine would be in the point five range. These values here, slip over one. This one gets a little higher. So I've taken the characterization that I've, with the calculations that I've done, are a little bit less than I would have achieved, had I used the NIOSH formula.

10 Q. Do you have any empirical data that you'd like to talk about that might illustrate the principals that are on this table, or that you've discussed in terms of the need to measure far below a standard in order to ascertain compliance?

15 A. Well, I'd say that that issue of the levels, the actual workplace levels in order to be in compliance, result in typical or average values falling well below it, which those data from the Simpson report, probably illustrate ...

Q. That was the data that Dr. Crump presented yesterday.

A. That's correct. I believe that's correct, since I wasn't here.

20 DR. DUPRE: Is this the data that is on page seventeen, figure one of the Simpson report, is it, volume one of the Simpson report?

I believe that the figures here are the same as we got from Dr. Crump yesterday.

25 DR. CHASE: Yes, the one that's in the pink file, yes.

I think it's an illustration of the practical implications of what we were saying from a theoretical point of view before, and that is that in order to achieve compliance with a given standard, one has to, the typical value is going to be well below that, that standard.

30 DR. DUPRE: Q. Can I just ask you a couple of questions about this table as an illustration of an empirical

DR. DUPRE: Q. (Cont'd.) example, the way you understand it.

5 So I look at the industry, which in this instance for the sake of the argument is the asbestos cement industry, and I see that basically, they had the results of eight hundred and forty-five different dust exposure level samples. Is that what the eight forty-five means?

10 DR. CHASE: A. I'm pretty sure. I'd have to go back and look in the discussion of the Simpson report to be absolutely certain.

15 Q. The thing that I'm also wondering about, is are we looking at a number of results for a single plant in the asbestos cement industry, or are there a number of different plants in there that have been sampled?

A. Again, I would have to go to, I was in terms of using it as an example, is just to illustrate that in fact it happened, but I'd have to go back into the Simpson report to see whether that - I'm just pure recalling it from that.

20 MR. SAMPSON: That pretty much completes my questions.

DR. DUPRE: If that is the case, I'm in your hands. Do you counsel, and the parties wish to proceed immediately, and then adjourn at one, or would it be preferable to you to rise now, and reconvene half an hour earlier than we will reconvene if we rise at one?

25 MR. LASKIN: Could I just confer with my colleagues, Mr. Chairman?

I don't think we have an awful lot of questions amongst us. Well, let's put it this way.

30 If the Commissioners are thinking of sitting past one to finish, I say one thirty or so, I'd say we carry on. If we're going to stop for lunch in any event, my preference would

MR. LASKIN: (Cont'd.) be to stop now, and come back.

5 DR. DUPRE: Well, why don't we rise now, and return at a quarter to two, one forty-five?

Thank you, gentlemen.

THE INQUIRY RECESSED

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10 THE INQUIRY RESUMED

DR. DUPRE: Counsel, are you ready?

MR. LASKIN: Yes, Mr. Chairman.

We've deferred to M. Casgrain, to allow him to make some travel commitments.

15 DR. DUPRE: M. Casgrain, if you would please proceed.

M. CASGRAIN: I have only two questions. I wonder whether, I think I should ask this one of perhaps Dr. Chase.

20 CROSS-EXAMINATION BY M. CASGRAIN

Q. You mentioned in the course of your examination, that in the counting there were several allowances had to be made, or certain allowances to be made.

25 If you're counting in a lab, which happens to be within the confines of a factory or mill, do you recommend that allowances should be made for the background noise of asbestos fibres?

30 DR. CHASE: A. No, and the first, I'll answer it in a couple of parts. First of all, one would expect that laboratory conditions would be such that whatever exposures might be in the plant, would be much lower in the laboratory,

5 A. (Cont'd.) but if it were located there, certainly the possibility of some pugative (sic) fibres being in the air, would be a realistic thing to happen, but to make that, that really gets into the whole issue of adjusting for blank counts, and how one handles that, and from a pure statistical point of view, as well as from a pragmatic point of view, my opinion is, to not make that adjustment.

10 Because, if for example you have some pugative fibres that might be in the laboratory, all that that would result in would be perhaps a slight increase in the count you might get, and therefore you're not in any way under-estimating anything. It would have only the effect of slightly increasing the count, and as such I wouldn't - and there are also some statistical reasons why the adjusting for blanks using a one or
15 two counts to adjust for many filters, is not a good thing to do.

Q. Another question, you testified to the effect that there had been some improvements over the last few years over the pump for instance, the mechanism being better equipped to have a continuous flow of air, and I assume as well,
20 that you will agree that microscopes have also improved over the last few years, microscopes used to read the fibres.

I don't know whether I'm asking the right person the right questions.

25 A. I think probably Dr. Rhodes can answer it from a personal point of view. I can answer it from what our industrial hygienists have told me.

I've looked through them, but I'm not a microscopist, but they've told me that, Yes, in fact that has been the case.

30 Q. And if that is the case, the two fibres that one was to read say fifteen years ago, and that we're still reading according to regulations as they still exist, are we

5 Q. (Cont'd.) reading the same thing, or are we improved, our reading, or are we if you compare the reading today, of those two fibres with present technology, with readings say fifteen years ago, how would you relate that?

10 A. The changes are such that, the best way I like to put that is, that if you were to have gone back in time, using today's technology to measure that same dust cloud, today's hygienist would have ended up with a higher number than the hygienist back then.

Q. And if I continue then if he comes up with two fibres, he is in effect, counting at a lower level than before.

A. There has been a de facto tightening of the regulation if you will, because of the improved equipment.

15 That would be a result of that.

20 Q. And am I carrying this too far if I said that you could end up with a situation where you would have improved the reading technology, if I may use the word, to an extent which would not have been followed by the technology in trying to eliminate the dust. You could come to a sort of absurd situation, could you not?

A. I guess depending on which one was advancing faster, that would be a ...

25 Q. And I suppose the obvious question now is, that I suppose that one has to consider technology not only in respect of what one can read, but also in respect of how one can eliminate the dust. Is that not correct?

A. I'm not quite sure ...

30 Q. All right, if I said to you, you know, you have stated that one could read with electron microscopy, and it has some shortcomings, but if one were to recommend reading say two fibres with electron microscopy, have you ascertained that one could have the necessary technology in its plant, in its mill, in

Q. (Cont'd.) its factory, to be able in effect to eliminate the excess dust, and be able to achieve the standard that this would call for?

A. One would have to say it would depend on what your counting rules were.

Q. Would you elaborate on that? I'm afraid that my question sort of threw me out there.

A. If you were to say with electron microscopy, let's take the extreme case, the transmission electron microscopy, where one could get the most refinement. If your counting rule were to say, Count everything you see with that, that's different than at the present time, when we have a three to one aspect ratio, and at least five microns.

Those are the rules under the present thing. If you were to change the , however you answer what the count might be, would have to be predicated on what the assumptions were in terms of the rules that you were following to count.

Q. Given the perimeters of counting, it might not change. Is that what you're saying?

A. Well, it's predictable that if you used the same criteria, that three to one, longer than five, then it certainly would be that you would get higher counts. You would count more, because the phase contrast is an index.

M. CASGRAIN: I have no further questions.

DR. DUPRE: Next, Mr. Laskin?

MR. LASKIN: I'll do my best to ask a few questions

CROSS-EXAMINATION BY MR. LASKIN

Q. Could I just come back to your Round Robin study for a moment, and just make sure I understand what was done?

I take it that all of the analysis was of

Q. (Cont'd.) chrysotile asbestos.

DR. RHODES: A. Do you want me to answer that?

5 The emphasis was on chrysotile asbestos in the range of zero to two fibres per cc.

However, we did not restrict it to that, because we wanted to get the other interfering materials, other asbestos if it were present, in as part of the variability that you would encounter in the field. But by and large, the samples were
10 chrysotile.

Now that is indicated in the data listing. Every one of those samples tells what the material was, and whether other material was available, or was likely to be present.

Q. I want to turn to that in a minute, but can you tell us where these samples came from?
15

A. I can give you a hypothesis. I repeat, we don't know who those companies were.

We the committee, the co-ordinator knew who they were, and it probably has been destroyed at this point.

Q. I see.

20 DR. CHASE: The data, not the co-ordinators.

DR. RHODES: A. The source of the data.

What we did, was get a very detailed information sheet, about three pages, which is also in the report, on each of those laboratories; their experience, their counter training, amount of material they, samples they'd counted, and that sort of
25 thing, and with that, we really didn't have to know who they were.

Now each sample has some detailed information on the type of situation that was sampled, so that you can reconstruct it, but basically, it was industry labs, I know from people who have told me that they were in it, there were some
30 consulting labs in it. There were several State Government

A. (Cont'd.) laboratories in it. Beyond that, I can't ...

5 Q. And they in turn would have got these samples from various asbestos workplaces, I take it.

A. That's true.

You should understand that all of the laboratories did not collect.

10 Q. I understand. Some of them were just receiving for the purpose of comparison.

A. Right. A number of them were not in a position to do the extra collecting burden.

Q. How did you know that you only had chrysotile, for example?

15 A. First of all, we didn't require that it only be chrysotile. Second of all, they were basically U.S. companies, and there's only a limited number of places that do not use chrysotile, that use anything but chrysotile, put it that way.

And each of the samples has an indication in the data sheet, of which type of asbestos they were sampling.

20 Q. Was there any analysis of the samples beyond using an optical microscope? I mean was there any electron microscopy that would have enabled you to differentiate the samples by fibre type for example?

A. No. Why would you feel that this was necessary?

25 Q. I'm not suggesting it was. I'm just trying to ascertain the origin of these samples really.

A. The origin of the samples - Oh, I'm sorry.

30 DR. DUPRE: Q. Just following up on the line of questioning here, do I gather then that it would or would not be possible for example, for you to construct a table like the one on page seventeen in the Simpson report, which would simply show the kinds of companies by aspect of the asbestos industry from

DR. DUPRE: Q. (Cont'd.) which you select the samples? In other words, how many from asbestos and that, how many from millboard paper, how many from friction materials, how many from textiles, how many from insulation?

DR. CHASE: A. It would be possible to characterize the samples according to the categories, and here I'd have to go to the legend for the data collection information.

DR. RHODES: A. One thing that you would have to be very careful of, is that these samples are not, and were not intended to be representative of conditions in the industry.

They were selected among the various work locations to give us results that were anticipated, in the zero to two fibre per cc range.

That was the area that we were trying to emphasize, but we wanted some in other areas, so that the counter didn't know that everything was going to be zero to two fibres.

We were not aiming at crocidolite or any of the other types of asbestos.

DR. CHASE: A. As to the type of operation that we had, I think it was nine, I have to go to the second half of the legend for the collection information, where it says, Operations Co OPR CDE (Ph) and that refers then to the operation descriptions, and then if you go to the collection data sheet, that the collecting laboratories turned in, there is in there, a section for those, for the type of operation, and I'm forgetting offhand now, what type of fibre introduction I think, grinding was another one, and there were some other types.

That type of a categorization would be possible, but with the, so in that sense, the table like the Simpson report, would be constructable, but the difference would be that that was all samples that had been taken by the factory inspectorate.

At least I inferred that. I read that description

5 A. (Cont'd.) and I get from that, that it was all of the factory inspectorate at those operations, whereas the ones that we would have here, could not be interpreted as being typical of those operations. It was just that those were the ones that they collected in response to the Round Robin collection framework.

10 DR. RHODES: A. In the same connection, let me call your attention to figure A6 in the appendix to the report, a sample collection worksheet. It's in the back, towards the back, in the appendix section.

Q. I'm sorry Mr. Rhodes, could I have the ...

A. Yeah, you're there.

15 Q. Oh, headed at the top, Illustrative Sample, Sample Collecting Work Sheet.

A. Yes.

20 Get down there towards three quarters of the way down, or towards the bottom, under Remarks, you have, Description of Operation, and there are eight plus another, which , each which was filled in for each sample.

It gives a fairly detailed description. It also says up above that, Type of Asbestos Present - Chrysotile, other, Possible Interfering Particulate, have I lost you.

Q. I'm with you. I don't know whether they are.

25 A. Let me run through it again, now that everybody has it.

About half way down the page, it says,

Q. Can we identify what this is first? Is this, there would be one of these sheets would go with every sample that you had.

A. Every pair of samples, yes, that's right.

30 Q. O.K., and would be prepared by the laboratory, or by the factory, or the manufacturing officer?

A. This is prepared by the industrial hygienist or whoever collected the paired samples.

5 • Q. O.K.

A. And all of that data, the results of this, is all summarized in the collection print-out at the end of the report. But the point that I want to emphasize, is that these are not representative samples of any particular industry, in the sense that they were selected to meet certain criteria as to
10 dust cloud type of asbestos.

They were not restricted to that, but they were selected for purposes of studying the analytical method in certain areas.

Q. What instructions went out to the laboratories as to what samples you wanted?
15

A. They were told that the study was intended to look at the application of the membrane filter method, for chrysotile asbestos, concentrations in the zero to two fibre per cc range.

20 They were welcome to send in samples of other material and other ranges, but by and large, to try and get samples in the range that we had specified.

25 The reason for this was we wanted to, we didn't want to specify it so tightly, that the counters knew for sure what was in those samples, and as you can - there's some of the data, I could pull some data, most of the results were in the zero to one range. And again, most of them were chrysotile, but there were some at around eight or ten fibres per cc. There was other material in there.

30 We achieved what we wanted. We had a sprinkling of other things to keep them honest, but most of the concentrations were the material, and in the range that we wanted.

Does that answer your question?

Q. Does that then account for what appears to be an odd number of actual filters that you had. I think I noted the figure one seven seven four. I mean, why one thousand, seven hundred and seventy-four?

A. Well, each laboratory had the option of determining how many filters they used to cover the full shift.

Some of the laboratories would use six or eight, and some would use one. They were asked to collect samples at twelve sets, I think was the collection number.

There's no way to tell what the number's going to be. It's a matter of how - Oh, go ahead Gerry.

DR. CHASE: A. Just take an example. The collecting laboratory, they would have twelve sets. The first one would be a worker selected, in the grinding operation, sorry. That one might have been where they judged it that in order to cover the eight hour period, they took four two-hour samples, so there would have been eight filters altogether. So that that set number one, during the first two hours, there was a left and a right, and then a left and a right, and a left and a right, and a left and a right.

So that one set could have contributed eight filters, or four pairs.

So the next one they selected might have been in another operation, where they judged they could take one eight-hour sample, so that they would get just a left and a right from there. So that's why, as you run across all the collecting laboratories, and all the ways we did it, you end up with a number that perhaps wasn't totally predictable as a nice multiple.

DR. RHODES: A. They weren't instructed to take four samples to cover eight hours at all times.

DR. CHASE: A. This got into the collecting industrialist's judgement of the situation as to what was an appropriate ...

5 DR. RHODES: A. You see, there's no criteria in the , no requirement in the NIOSH criteria as to how many samples you take to cover an eight hour time period, beyond there were certain fibre densities on the filter that you're supposed to try and achieve.

Now we counted whether we got that density or not.

10 Q. So I take it then that what you got from a collecting laboratory, were a number of filters, and a particular count that went along with each filter. Then you tried to assess what variation there might be in that count if you sent the same filter to another laboratory, or to somebody else who was counting it within the same laboratory.

15 DR. CHASE: A. Right, but let me, it's important to note that when they took these sets, let's take this one in particular, both filters went into the co-ordinator, and one of them, a randomly selected one, was sent back with a blind coding on it, to the laboratory that collected it.

20 Now they couldn't be sure that they collected it, because along with the one they collected, there was another laboratory over here that collected some, and they also received some filters from other collecting laboratories.

DR. RHODES: A. We were trying to achieve double blind. I don't think we completely succeeded, but we at least made made an effort out of it.

25 They didn't know that they were, they weren't sure that they were counting their own samples even when they got their own samples back.

Q. But given what you just said, Dr. Chase, and with that caveat, is the answer to my question, Correct?

30 DR. CHASE: A. What you said was basically, Yes.

Q. O.K., and I take it from what you said earlier

Q. (Cont'd.) this morning, that the laboratories that were chosen, or that agreed to participate in this study, there was no prior selection of those laboratories on any quality grounds, other than the two criteria that Mr. Rhodes suggested.

Do we know anything for example about the quality of the people who were counting the filters.? Do we know how well trained they were?

DR. CHASE: A. We have characterized on there; how many years they have been counting, and what their source of training was. Whether or not they had been trained by an outside agency, such as a NIOSH training course, or whether they were in-house trained, 'cos quite often, you'll get the elder statesman of the laboratory was the one who was trained by say a NIOSH training course, and subsequent to that, when new industrial hygienists are added, they are trained by the one who was trained.

So we tried to characterize not only the length of time that they had been counting filters, that experience factor, but also we have recorded the information, collected it, as to what the source of training was.

DR. RHODES: A. Now the data were analysed in terms of least experienced, and most experienced counter, and since there were really no inexperienced counters in any of the laboratories that were involved, we found no great difference, nosignificant difference, regardless of which counter you selected to represent the laboratory.

Q. Is there in fact an analysis of the data, I mean in a collective form, in the same way that you were presenting some of your theoretical data this morning?

DR. RHODES: A. Yeah, there are extensive tables in the report.

Q. And can you, because perhaps it's me that I haven't - I know there is all the collection data, but is it all put together in some analysis, where you can look at it - coefficients of variation, you can look at ninety-five percent confidence intervals, and so on?

DR. CHASE: A. Well, there are two, a couple of answers. First of all, there are some graphical representations in chapter seven, in the presentation of results, where the figures give one collecting laboratory's determination on one axis, the paired laboratory's determination on the other axis.

There are several, and let's be more specific about it, going to those tables and figures at the end of chapter seven ...

MR. SAMPSON: That's figure seven four, to figure seven six. Is that right?

DR. CHASE: Yeah, in figure seven four, through seven nine really, are different graphical representations of the TWAs.

DR. RHODES: A. The CVs are all listed prior to that.

DR. CHASE: A. And the figures seven point one through seven point three, and we were looking in detail at seven point three this morning, the analysis of the data resulted in those data points that are presented on those figures, in those figures.

The, if you will, I think you use the word theoretical, 'cos I was using it, those calculations were then using these results to carry further the discussion along the lines that we were going this morning.

Q. Well, just let me make sure I understand that. The tables that you put up this morning, and the figures that you put up this morning, 'cos I may have misunderstood the

Q. (Cont'd.) evidence, were they then based upon the actual results of your study?

DR. CHASE: A. Yes.

Q. O.K., I'm not sure I understood that.

DR. MUSTARD: Well with that application, I asked if those were theoretical numbers in those tables.

Actually, they were theoretical calculations.

DR. CHASE: The two axes, the ones that led to the things, the one axis from the CV for the CV of the thing, would have come from the calculations here.

The other axis of the geometric standard deviation, came from the NIOSH data, but then, you're correct. The numbers that were in the table itself, were then a result of a theoretical calculation.

DR. MUSTARD: I'm referring particularly to table one in the material, tab eleven is it?

MR. LASKIN: Yes.

DR. MUSTARD: Table one in tab eleven, and table eight one in the document we're referring to. I'm left with the impression at the moment that the calculations in there are theoretical calculations.

DR. CHASE: That is correct.

DR. MUSTARD: I think counsel, that should be sorted out then, so that the actual data on the overhead display we had, with the green numbers on it, those were real numbers, except for the calculations in the body of the table.

The table one in tab eleven, and eight one in tab whatever it is, those are theoretical. They are not - there's no live data in those.

DR. CHASE: Yes, thank you for that clarification, 'cos I was really talking about both.

MR. LASKIN: Q. I guess what I'm, and it's my

Q. (Cont'd.) ignorance of this whole field of statistics no doubt, I guess what I'm really struggling to find out is, you've got a particular filter, or pair of filters, and you circulated it around to various counters, and what I'm really trying to ask is, did you then assess what variation there was in the counting of either that one filter, or the pair?

DR. RHODES: A. Those are expressed in the CV tables and figures. Those are the direct results of the - not in those, in table seven five for example, is the basic summary of the results of the study.

Those are the coefficients of variation, calculated from the results on the filters. Those are individual filter results.

Q. Right.

A. Now the figure that I can see you looking at there, we also calculated the TWAs for each set of samples, and they're compared in the figure that - in figure seven four through seven six, seven seven, seven eight.

DR. CHASE: A. Could I take my example here, and just carry it through how these would show up on these tables?

MR. LASKIN: Q. Sure, it would help me.

A. Part of these would come back to that laboratory, part of them would go out to another laboratory. The set by the way, may remain together, and of course likewise so every pair of filters was evaluated like two laboratories.

Now when we were estimating the coefficients of variation, which, well the actual numbers of those from the estimates, based on the data received from the laboratories appear in table seven point five, and figure the seven point one through seven point three.

O.K., we've got - the tabled values that we've just graphed in the other papers.

DR. RHODES: A. There's a figure in a table for every point on those graphs.

5 You're looking at the wrong one again.

MR. LASKIN: Am I? I've got the wrong ones?

DR. CHASE: A. I'm looking at figure seven point three.

MR. LASKIN: Sorry.

10 DR. CHASE: A. And the numbers from these individual pairs of filters, would result in the estimates that are plotted those points, the figures in figure seven point three are the graphs in seven point three.

15 Those points that are plotted also appear in the tables in chapter seven, so there's just one place as a table, and then there's a graph of those points.

Those came from the actual raw data that were received back from the participating laboratories.

20 Then, now turn to the figures seven point four, through seven point nine. Based on this set of filters here, the one where I said hypothetically we had four sets of filters, these four filters right here, had they been returned to that original laboratory, would result in a time weighted average estimate for that worker.

25 These four filters might have gone to a laboratory over here. We receive them, and we could also calculate a TWA. And those four together, would result in a single number for this, a single number for this laboratory, and those are the numbers, the pairs of numbers that are plotted in those figures, seven point four through seven point nine.

30 The reason for doing both individual filters, and plotting TWAs, is the assessment of the uncertainty inherent in a filter count has to go down to the filters. Possibly that's the way it's been done, and furthermore, that's the only way that it

5 A. (Cont'd.) lends itself to the analysis that we're doing, but the bottom line of course, is the time weighted average, the estimate of the average exposure over that day, and that's the reason for those plots, to simply show you the spread of the data that we would get from one laboratory counting one pair of, the left filters if you will, and another laboratory counting the right filters.

10 DR. MUSTARD: Q. Can I ask you a question about those plots then?

This may not be fair, but I remain for reasons that I may get into later, quite convinced about the fact that when we do all this measurement, you end up with a log normal distribution.

15 I'm just looking at those figures since that measurement is part of the calculation of the TWA, so that distribution probably affects the TWA distribution, which probably has a log normal type distribution.

DR. CHASE: A. It would be certainly something that's similar to it.

20 Q. Therefore if you logged those vertical and horizontal axes, you would create a very interesting effect. You would make the data much more symmetrical.

That is, you would bring the outriders in, and you'd spread the bottom out, and I wonder if that wouldn't be a fair way to plot it some time.

25 A. That would be an interesting plot on it. The only thing is that it would be, if we were sampling from a similar situation, the fact that these are coming from quite a number of different collecting laboratories, in different situations ...

30 Q. O.K., then I think I'd better bring in my point if I may counsel.

MR. LASKIN: Mmm hrm, by all means.

5 DR. MUSTARD: If I may go into another
biological field for a moment, but one all of you will understand,
because all of you have experienced the fact that your blood
will clot if you take it out of your vessels, and expose it to
your skin or a table, and it takes a certain amount of time to
clot. And if you do a hundred samples of measuring how long it
10 takes your blood to clot, plot the data, and do a distribution,
you'll find it has a log normal distribution.

Now, if I take a sample of blood from you, and
systematically dilute out key enzymes that are involved in the
process of clotting, I will lengthen the length of time it takes
your blood to clot, and if I plot that out of degree of dilution
15 of a key enzyme versus the time it takes to clot, it takes on
exponential functions, which of course becomes a straight line
of the logarithmic transformation.

Therefore in a sense, the biological process is
reflected in the individual measurements of plotting times,
ending up with a log normal distribution.

20 You see that analogy? Now as I listen to the
presentations here about fibre concentrations, and fibre particle
size distribution, I become convinced, if you could measure all
the particles that are generated in an asbestos mine, or cement
factory plant, then indeed those particles, discounting particles
25 have a log normal distribution.

You're sampling out of that particle distribution.
Therefore it doesn't surprise me that mathematically, you end up
with your measurements having a log normal distribution.

Therefore, I do not think it would matter where
the samples came from, that the logarithmic transformation would
30 hold.

DR. CHASE: A. You may be right, and the key would

A. (Cont'd.) be the coefficient of variation of that log normal.

5 DR. MUSTARD: So I just think that those plots might be much more reasonably presented as log log plots, and under those circumstances, it would look a little different than it is. There'd still be obviously variation between...

10 A. Yeah, I can't disagree with what you're saying. The only reason for presenting them the way we did, was to give the actual scale that we were working on.

DR. UFFEN: Q. Would it not also have the advantage that you'd be able to see the relative difficulty of measuring at point one, point two fibres per cc?

15 They're all jammed down there in the corner, and you can't tell whether one lab varies much from another. On a log log plot, you'd be able to see it.

DR. CHASE: A. Yeah, your point's well taken, and we attempted to get at that, by at least blowing up that lower portion, that two by two portion of the zero to ten plot, by going zero to two.

20 DR. RHODES: A. Some of those are zero to ten, and some are the same thing zero to two.

DR. CHASE: A. But there's nevertheless, there is a bunching up.

DR. UFFEN: Oh yes, number seven five.

25 DR. CHASE: Yeah, but, your thoughts are ...

DR. UFFEN: See, it looks at first glance as though the co-relation is rather good down in the low. Put it another way, the scatter around the central line down in the lower levels doesn't seem to be as bad as when you get out further.

Is that just ...

30 DR. RHODES: A. Well percentage wise it's ...

DR. CHASE: A. I think in part it's, we've got a

5 A. (Cont'd.) lot of values down in there, but then it's not only looking at the scatter about the line that you sort of have to look up in vertical strips, and horizontal strips to see how these things pair up.

10 But then the discussion of relative error rather than absolute error necessarily comes in, and the suggestion of a log log plot, a plot to illustrate that point, I think is good adjustment in doing this.

10 DR. RHODES: Never seen it plotted that way, this type of data.

MR. LASKIN: Q. Did you find any significant differences between the results with the more experienced as opposed to the less experienced counters?

15 DR. CHASE: A. Well, that analysis was done first, and there really was not an appreciable difference between them, but not all laboratories of course gave replicate counts on the filters. That was a volunteer thing on the part of the participating laboratories.

20 Some laboratories returned two or more counts for a set of filters, other laboratories would have only done one. And because of that, it would be improper, both intuitively, I think, and from a rigorous point of view, to take two counts from the same laboratory, on the same filter, when other places were only taking one. So we had to come up with, how do we select that where we did get more than one count from the laboratory, how do we select the one to use?

25 So one of the thoughts that occurred, was how about the most experienced counter? And doing the analyses with the most experienced counter or the least experienced counter, supposedly two ends of the spectrum here, in some sense, you couldn't, it wasn't distinguishable in that sense, so we made the decision to present where there were more than one count from a

30

A. (Cont'd.) given laboratory, we would take the most experienced counter result.

5 Q. Can I ask you, were all of these counts done when, in 1977, 78?

DR. RHODES: A. Basically through 1978. We could reconstruct the time frame, but they were done in a period of approximately a year, through 78.

10 Q. Can I ask you this from your own knowledge, do you believe that either there have been any changes in technology for equipment on the one hand, or do you believe that for example, implementing your AIA membrane reference method as opposed to the NIOSH method, either or both would have any effect on minimizing or reducing the amount of variation that you got in your study?

15 DR. RHODES: I would think the, I would hope, put it that way, that the AIA reference method will have some effect on reducing the variability. We set up some rules. Now when you're talking variability, you have to decide whether you're looking at variability between countries, or variability within a country, and that method was primarily designed to at least try to get variability between countries organized.

20 NIOSH to the best of my knowledge, has not implemented, there has been no change in the U.S. official counting procedures. I know they do use the preparation method for their p'tesides (ph) and that sort of thing, but by and large there has been no change in the U.S. method since this procedure was used. They published it in a formal way, but it's the same procedure.

Now as far as the, I'm talking about the counting step primarily here, ...

30 Q. Is that now, I was just going to ask you if you believe your AIA method is an improvement on the NIOSH method.

Q. (Cont'd.) Is that where you mostly suggest it is an improvement?

5 A. Yes, there's a series in here that we suggest places that could prove it. Now the big source of variability that very little has been done on, as indicated by this study, is the sample collection step.

10 There have been some significant improvements in pump technology in the last several years, which I think will, would hope would help in the collection step, but this question of where do you put it relative to the dust cloud, there is really no guidance, and no standardization. And I keep coming back to that as the area that we really need to see what can be done in that range. We really didn't address it here, other than point it out.

15 Q. Does the AIA method endorse it?

A. The AIA method specifies that the sample be in the breathing zone within a fairly wide range. They do not address that particular problem in any great depth.

20 Q. And is that in your judgement, the main source of variability in the sample collection process?

A. I think yes, basically. I don't think the pump calibration, and that sort of thing is a particular problem.

25 It's trying to get a representative sample from the dust cloud, and we're looking forward to seeing what NIEHS has done. They made a study of that particular aspect in some other, using some other contaminants.

Q. Leaving aside the sample collection process for a moment. Are your results on variability with your sample evaluation part of the process? Are they reasonably consistent with NIOSH's results?

30 A. That's an interesting question. I don't know how deeply you want me to go into that. NIOSH used the Johns

A. (Cont'd.) Manville study as the basis for their revised analysis of the variability of the method.

5 First of all, they took the position that the within-laboratories random variability, plus a very small additional amount for the collection step was the total variability of the method. I think we disagree rather strongly with that position.

They also in analysing the data, went through a statistical ...

10 Q. You disagree with that because of the small additional ...

A. I don't think, I think they are not coming even close to including all the sources of variability in the method.

15 Q. O.K., I'm sorry, I didn't mean to interrupt you.

A. I'm sorry. They largely eliminated the collection step, by adding a small variability.

Gerry, you look primed. Why don't you do it on the blackboard?

DR. CHASE? How much time have we got?

20 No, the reason that it was the J.M. data that they analysed, and of course I'm very much familiar with the J.M. data on that particular study, but I think I can - it has to do not with , so it was the analysis, and how they analysed the data.

25 If I could very quickly, as quickly as I can do it this way, describe what we did on that particular study, to show you why the analysis did, just to at least give you an intuitive feeling.

30 We collected simultaneous samples not on workers, but fixed place settings, so we could get six at a time. We built a little stand, and put it - we asked our hygienist to give us a spectrum of experiences in the workplace, and so we did six simultaneous filters, and some of them might have been a series, like we had before. One of them might have been one all the same

A. (Cont'd.) time. And one filter went to each of our five laboratories.

5 Right, then if this laboratory had five individuals in it, they each cut their own slice, mounted it, used the microscope in that laboratory, and counted it.

10 Likewise, if this one here had three people counting it, and we had varying numbers, depending on the laboratory, all three of them independently cut a slice, mounted it, counted it, and analysed it. Likewise for the other filters.

So what you have is then, within-laboratory, within-filter information from this study, and you have within between-laboratory, between-filter, inter-laboratory, inter-filter.

15 When we analyse the data, we use all of the information. When NIOSH analyzed the data, they didn't compare the count on this filter from this laboratory, with the count on this filter from this laboratory. They only compared within-laboratories by themselves. They didn't make all of those types of comparisons that were possible from the data.

20 So one goes back and looks at it systematically, and there's a you know, very logical explanation why one would arrive at a lower value.

25 There's some analytical difficulties as well. That issue I mentioned before about we like to use techniques that are like the jackknife that are applicable under a variety of situations. It appears as if they sort of had a screwdriver to try and cut something with, to stick with my analogy from this morning.

Q. Can I just ask you one or two more questions about this topic, and then I'll leave it, and let me ask another question that'll show my ignorance of statistics,

30 In your argument this morning Dr. Chase, one of the factors you put up on the board, in one of your two factors

5 Q. (Cont'd.) was inability to get below point one fibres per cc. I'm not sure the exact language you used, but I take it that was the sense of it.

A. Yeah, basically I think I used the phrase, Inability to distinguish between values below there.

10 Q. And are you, when you say that, are you referring to the inability of the optical microscope to measure below that level, or are you meaning something else?

15 A. The whole thing that we're calling the membrane filter method, from the collection to the analysis, that umbrella statement.

20 Q. And I guess what I'm having some trouble with, is if what you're talking about, and what you're concerned about is not exceeding a particular level, why does it matter that you can't get below point one? Why can't you simply call all of those values down at that area, point one or less?

25 A. That's in fact what we do, in terms of reporting results. But the analysis is such that in order to, with the table that I've presented, the analysis is such that in order to make the statement with a degree of reliability that it falls you know, with ninety-five percent confidence that no more than five percent of the values would exceed it, I have to be able to distinguish between a point one and a point 0 nine, and a point 0 eight, and a point 0 seven.

30 MR. LASKIN: Right. My Commissioner is going to clarify my ignorance.

DR. MUSTARD: No, I'm going to try to display my ignorance as well.

35 DR. MUSTARD: Q. Let me pose this a slightly different way. If I had a system developed, that would sample and count to give me sufficient numbers of fibres, let us say I would count a thousand fibres, I realise there may be technical

DR. MUSTARD: Q. (Cont'd.) reasons why I can't get that through your sampling, I count a thousand fibres, then you'd probably have no problem at all, in differentiating between point one and point O five.

In other words, am I not right, it's that your real problem is that the number of fibres that you can count with the existing system to get a reliable index at these low levels, is that too small a number of fibres will actually be counted into the calculation.

Is that not what the problem is?

DR. CHASE: A. Well, that's part of it. You're absolutely correct in that I would know more about that sample if I were able to get a much more extensive count of the filter, but the data that we have suggested, there are other factors going in there, such that even if we were to extend the count, those other sources of air are still going to be there of a magnitude that we're going to have the problem.

If you counted more of the filter, you would improve it, no question about it. But you wouldn't be able to improve it to the point of - you would still have some place in there where you're going to run into trouble, because of these other sources of error that are there.

In other words, sort of an intuitive, if one laboratory is coming up with a count of twenty, and another laboratory is coming up with a count of seven, just for example, the available evidence would suggest that even if we were to extend the counting rule, to have them count so that we're not at twenty, that the higher one is out around the thousand, or something like that, that there would still be a fairly large error between the two that would not be corrected by the thing that we know we can improve, by counting more.

Q. But we don't know that because we haven't been

DR. MUSTARD: Q. (Cont'd.) able to test that in actual practice, have we?

5 A. Well, we haven't been able to test that specifically, you're right there. But what we can do by separating out some of these, if you're familiar, like with an analysis of variance type of thing, where you try, that's a statistical way of trying to separate out some of the sources of error. And what we've been able to do with that, would suggest
10 that counting, even if we were to count virtually the whole filter, we would still have a problem with differences.

Q. True, I guess I was designing a different method, getting all the fibres in, but to come back to the counsel's point, my interpretation of that statement that you have in your text, in tab eleven is it, is that what it is, tab
15 eleven?

MR. LASKIN: Yes.

DR. MUSTARD: Q. The one we got yesterday?
On page twenty-two, using this method, the second
paragraph,

20 "Using this method, the lowest asbestos standard which permits the possibility of achieving the desired degree of confidence, without requiring measurement below zero point one fibre per cc, falls in the range of - "

25 But my interpretation of that would simply be, when you got fibre densities which give you very low values, so what? They're low values, and that's indeed, that's a desired part of the story. We know that it's not going to be beyond an upper boundary based on your calculation, and this'll seem to me to say that you shouldn't set the fibre standard at zero point
30 three fibres per cc. You just feel that should be an adjustment in pushing it down.

5 DR. CHASE: A. Well, the only thing is, you could not then have the ability to measure, to reliably assure yourself that you're not gonna be above that standard.

DR. MUSTARD: Q. O.K., well then I would suggest that maybe I'd word my legal document something like this.

10 Ninety-five percent of the readings must be less than zero point five CCCs and you know, that's when it doesn't matter. It's when they go above that, that if I set my control point at the upper end of your boundary, at your confidence limits. Do you see what I'm getting at?

15 A. Yeah, but I still - the statistical problem of not being able to if you will, me as the person who's doing the sampling, as the industry who's doing the sampling, cannot reasonably assure myself within the capabilities of the method, that I can do that.

Q. Well now, I don't quite follow the logic of that.

20 Let's go at one cc, one fibre per cc, all right? And if one set the rule that the upper limit, the upper limit, I'm not saying what the time weighted average should be, what the upper limit should be, must be on ninety-five percent of readings, one fibre per cc or less.

That would, by your tables that you gave us, achieve a time weighted average of zero point five.

25 A. You would achieve something below it, and in order ...

Q. Something in that range, something in that range, and that certainly you're going to be able to count one fibre per cc, and therefore those lower levels don't really become very important to me, if I look at it that way.

30 I realize that may be pretty impractical from the records here point of view, but that's another way of

DR. MUSTARD: Q. (Cont'd.) interpreting all that data. I could rewrite that paragraph that way, and I'd still be saying the same thing.

A. Well, you still wouldn't, if you're going to get into that NIOSH concept of being able to assure oneself with some degree. You then enter into not the kind of calculation we're doing there, but rather a binomial situation, each sample is or isn't. Now it takes a large number of samples to begin to assure oneself that that would be the case, and I would suggest it would become impractically large.

Q. But still, it's another way of writing that paragraph.

A. It would be.

Q. It's the practical aspects of the application's another question, but it's another way of looking at the same issue.

A. It would be another way of writing something related to the paragraph, but I would have to say, that paragraph couldn't be quite rephrased in that.

Your point, and I understand your point, but it wouldn't be quite - you've added it, you've not put it in the same context as the paragraph.

DR. MUSTARD: Sorry, counsel.

MR. LASKIN: No, that's fine, you did it much better than I could have done it.

MR. LASKIN: Q. Just let me ask a different question along the same line, and tell me whether I'm way off base. I mean, why can't you for example, hypothesize that all of your counts, that if you can't get below point one, why can't you simply assume that all of the filters in which you don't see anything, you give them an arbitrary count, which is either point one, or let's take a midway point, point zero five.

I mean, you know, we're talking about indexes to

Q. (Cont'd.) start with. Why can't you do something like that, if it's necessary to have a precise figure for your statistical calculations?

A. Well, if you did what you're saying, 'cos I guess in a certain sense that's the way that many of the data are recorded now, and that is, if it turns out that the calculation result's less than point one, less than point one is reported. But to say that to arbitrarily - to report it that way, I have no difficulty. It's just that now, if you're going to use those data to try to say something about, with some degree of liability, what that worker is exposed to on many days, that five percent of the days kind of thing, that the bound you end up with is going to be pushed up.

Those lower numbers that we get at, with all the caveats that go into how they were calculated, are the bounds that you begin ending up with.

So I mean what you're saying that you can do, I'm just, what I'm saying is that in order to do that, under certain conditions for CVs and the coefficients of variation, and the geometric standard deviations, they would be incompatible with some standards in there.

Q. Yea, I think I understand what you're saying. It's just that we start out with an index it seems to me, which is in many ways is just that, an index. And we've heard lots of evidence that basically it's measuring very few of the fibres that a person's actually exposed to, and you know, which raises the question as to whether it's the right index, whether it's an index of the right thing.

I just don't see why you can't add another assumption into the system, if what you need is another assumption to make your statistics work.

A. No, see, that assumption is simply a recording

5 A. (Cont'd.) mechanism. The problem that would exist with the evaluation as we presented it, remains the same. That doesn't get around it, it just lives with it, if you will, and the result of that would be that you would not be able to make that kind of statement with many standards, because your ability to say the statement is above at the very least, above the standard.

10 Q. Can I ask you just a different question? It's the last question on this point.

Do you reduce your variation if you go at your standard in a different way, and I'll try a different way from Dr. Mustard's proposal?

15 But instead of setting a maximum permissible exposure limit, suppose you set a standard based on the fact that for example, a worker is subjected to I don't know, ten samples a week, and the average of those samples, we'll say, should not exceed a particular level, let's say one.

20 Can you reduce the amount of variation in the system by positting a standard which is based on not exceeding an average over a particular period of time, rather than worrying about whether one particular sample exceeds the limit?

A. Well, are you saying from a practical, or a theoretical point of view?

Q. Let's talk practical, in the workplace.

25 If you define the way in which you set your standard differently, so that you don't have to worry about the inspector who comes in, and one times in twenty finds that, you know, the worker's got you know, two point sevens instead of two, or one point three instead of one. Instead you say, ...

30 A. You're scrapping the one day, excuse me for interrupting.

You're scrapping the one day compliance, non

5 A. (Cont'd.) compliance framework, and replacing it with something that would average over several days, and require that when that assessment is made, that you do it that way.

Q. Sure, but let's say he's sampled once a day, and every week, the average of his exposure should not exceed the upper limit.

10 DR. RHODES: A. You're talking a grab sample once a day, or full eight hour sample once a day?

Q. Let's talk theoretically for a moment; a full sample.

15 DR. CHASE: A. So essentially full, if you get into the full time monitoring for every worker, then we'd have to bring in a practical limitation.

Q. But in theory, could you not reduce the variation in the system of doing that?

20 A. Oh yeah, from the theoretical point of view, yes. The minute you bring in more things, and begin to average, then when we average, some of those contributing errors begin to get reduced.

DR. UFFEN: Q. Could you increase the flow rate in order to get a larger number of fibres to count? I notice it's two litres per minute. Have I got it right, per eight hour shift? Two litres of air through the pumps.

25 DR. RHODES: A. There are a couple of considerations there. First of all ...

Q. What about ten times? What happens?

30 A. Well, first of all, you can't carry enough batteries to run it that hard that long. You also, let me think a minute, there was a fair amount of work done on the DMAP on the effect of the face velocity on a count, and I will admit we didn't come to any final - clearly demonstrated, but it looked like there was a range below which you shouldn't go, and a range

A. (Cont'd.) above which you shouldn't go.

5 DR. UFFEN: Q. Two litres per minute seems to me intuitively as very slow. I'm not sure whether it is or not, never having worn one of these.

A. What we recommended, was a different, change reducing the filter size, to make pumps smaller.

10 You also, as you go up in flow rate, you tend to load your filter faster, the pressure's up.

Q. you tend to load the filter faster, but what does that do?

A. It increases your pressure drop.

Q. Is that bad?

A. It infects on your pump.

15 What I'm really saying, I think, is that the limitations on your flow rate, is primarily your pump, your ability to pump the material with a device that can be worn.

Q. O.K., maybe it's a technology that's reached it's limit.

A. Pardon me, there's one other point.

20 Q. It starts to vibrate?

A. No, the flow rate and the face velocity was also selected to approximate the tendency, the reparability of the fibres. You're not running a cyclone in front of it the way you do with dust, with respirable dust. In other words, it in effect gives you a respirable nonrespirable cut, without going
25 through the problems with the cyclone, which gets to be a little messy with fibres.

Q. I can see the point of that. If we were concerned about trying to reproduce the kind of measurements right where the workman is, you don't want to have them widely different from what he's actually breathing under normal circumstances, but if
30 we're trying to regulate and monitor in a region where we know the

5 DR. UFFEN: Q. (Cont'd.) rates are low, that's what the knowledge is in the first place, then maybe it's not so important to reproduce the precise conditions that the workmen have. Up the flow rate maybe not ten times, but double it.

10 Presumably that would increase the number of fibres you would be able to count in an eight hour day by a factor of two, and if you could triple it, you might be up to a region where you could measure point one fibres per cc, not by trying to improve the optical counting, just by getting more fibres.

15 DR. CHASE: A. Certainly from a theoretical point of view, what you said would be something that could be investigated. One way I'd have to answer is that that hasn't been done per se, so that one couldn't answer exactly what it does to it, and I would you know, the empirical data would sort of show from a study if one were to take that kind of a situation, what might happen to it.

20 Harry, would Steve Becket's investigation - I'm being vague about an article that appeared in the literature in the last couple of years by Steve Becket, because he addressed some of the collection parameters, and I can't recall specifically, but I think that one of the ones was the thing that we're addressing here, and I think I'm correct, in that again it was consistant with this, "You don't go below something, and don't go above something because things happen."

25 DR. RHODES: A. It's not certain under the conditions that he's described, that you've described, that a very low concentration that's going to a higher flow rate to get more fibres, in theory might be the way to go.

30 On the other hand, the tendency has been to make the pore size in the filter larger, to reduce pressure drop. To make the pumps smaller, it isn't easy to get a pump that'll run

5 A. (Cont'd.) eight hours at a constant rate, and for personal sampling, particularly when you begin putting this automated equipment in there to check out whether it's been tampered with, it's a pump design problem to get a personal sampler that'll run at those much higher velocities.

10 It has, Gerry says it has not been done. The tendency has been in the other way, to make them smaller, and make them more constant.

MR. LASKIN: Q. Could you just elaborate on a statement that is at the bottom of page seventeen of your text, which is tab eleven?

15 Will you talk about the difference between multiple sampling, and a single full shift sample in an eight hour period?

DR. CHASE: A. The thing that we're, that that ...

Q. Is there a policy suggested in this?

20 A. There are certain errors that are going to be in much the same way that you were suggesting before. Suppose we take an average over several days, would that reduce in some way the variability? And the answer was, Yes.

25 That same phenomena that you're talking about, there are errors contributed by the analysis of those filters, and if you analyse a few of them, you're gonna begin to average some of that damp down, some of the fluctuations that one would expect.

So this is just a statement that the data we have from the Round Robin, would suggest that you do see that kind of a gain, by taking more than one filter.

There is one point to keep in mind. We say suggested, and that is ...

30 Q. Is it practical?

A. Well, no, it may or may not be practical, I

5 A. (Cont'd.) mean in terms of taking, instead of, if eight's better than one, let's always take eight. That involves mounting and counting eight filters.

10 And the other one is just a footnote, and that is that the between-laboratory comparisons that we have from the study, were as a result of a solid randomization between laboratories, and therefore the bottom line main inter-laboratory, inter-filter variation that we have, you know, very comfortable, very reliable, as far as I'm concerned, because of all of the scientific design that went into it.

15 It's when you talk about the information that we have about repeat counts on the same filter within a laboratory, the intra-laboratory, intra-filter. That did not come from all laboratories, that came from those who volunteered to send in more than one count.

20 Therefore the randomization that we got from the other comparisons that we have, was not present in that, and so part of this, the conclusion that we say why one can reduce it, is based on the data from those laboratories that did volunteer to send in more than one count. But the randomization, the number of laboratories who actually sent it in was fewer than forty-six quite a bit, so it's just that, I would just say that that's the idea of taking, you do begin to see the effect that you alluded to before by even within a day. But there would be a lower again, a point below which you wouldn't gain, you'd begin to lose.

25 Q. Can I just turn briefly to one topic which you raise in your text, although I don't think we discussed this morning, and that's the use of the scanning electron microscope? And I raise it only because we've had one of our previous expert witnesses, who at least suggested to us, that it was indeed a feasible instrument in the workplace, and I note from reading your text, that you appear to take a contrary position.

30

5 DR. RHODES: A. It would depend really on what you mean by feasible. Electron, my experience with them has been limited. We've done some work using them, but I've never operated one myself personally. I've stood there and counted on them.

10 Basically you're talking something like seventy-five thousand for an instrument, and you're talking a rather skilled operator. He's not somebody that you're gonna be able to bring in off the street generally, and train as a technician.

15 The other, there are a number of other problems. Are you going to , first of all, what kind of filter are you going to collect on it, because all the membrane filter, as the name implies, is collected on membrane filter, and you cannot count on an SEM on a membrane filter, unless you make some preparation to get rid of the sponge structure. You're literally looking for a needle in a haystack.

There are some techniques that we've done, some acetone reduction, I think this triacetone acetone thing will do it, but I've never done it personally.

20 The alternative is to collect on a nucleopore and that's got all the little holes, and you have no assurance that you've got a uniform collection, and there are also some problems of having the materials that you collect in the field, back on the same filter, when you get it back in the laboratory.

25 You also, there's a question of what magnification you're gonna count at. The work we did was at five hundred and a thousand, which was just a better resolution, with some chemical analysis availability.

30 If you start going to higher, two thousand, five thousand, you begin to get into the problem of your field area gets smaller and smaller and smaller, and you're counting a much smaller number of fibres.

So, what we, an area that I would consider practical

5 A. (Cont'd.) is to use the electron microscope in a very selective manner to characterize a dust cloud, for example, identify what some of the things are in it, get a proportion of fibres of a given size range which are asbestos, and apply those kind of corrections to a group of samples from the same location; something of that nature.

10 Identification on it is also not as easy as some people might imply. You see something, you know darned well it's asbestos, you can't get it to give you the right peaks.

15 Really the bottom line is, it's an expensive, relatively slow way to work, requiring a level of skill which is considerably higher than for routine counting on the optical microscope. So I would think it might have some selective uses, but as a routine procedure, I'd hate to have to start counting everything on an electron microscope.

DR. DUPRE: Q. Just to tease this out a little bit, I'm thinking of where the electron microscope can be a useful supplement to the membrane method.

20 Can I take it both from what you've just said, and from what is in this document, especially on page ten, that you're thinking of the SEM as complementing the membrane filter method?

The SEM really cannot complement much beyond, as you put it, characterizing the workplace environment

25 DR. RHODES: Well you can get considerably higher magnifications on an electron microscope than you can on the optical microscope, but as soon as you do that, you've lost field area. You're paying for something. It's a trade-off, is what I'm trying to say.

30 Q. Now what you've just said there, I suppose, is also a reason why I imagine as you pointed out on page eleven, there is indeed no known relationship between SEM counts, and optical measurements.

5 A. That's basically true, if you count on the SEM applying the optical counting rules at the same general optical mag. You can get a fairly good result, at least that's been our limited experience.

Didn't Danielle report some of the same, at one of the meetings, do you remember?

10 If you're counting the same general size fibres at the same mag., you can get some fairly good relationships.

DR. DUPRE: Q. So just on the basis of what you said then, would this encourage me to discount a view that I have heard, which is that the preparation of the sample, for electron microscopy may perhaps affect the size and dimension of the fibres being sampled?

15 DR. RHODES: A. Are you talking SEM or TEM, scanning, or transmission?

Q. I was talking scanning.

A. What type of preparation were we referring to?

20 Q. I would have to go back to the testimony from the particular witness involved.

A. Well, if you're going to use the nucleopore, all you have to do is code it. If you're going to use a membrane filter, you've got to do something to destroy that filter structure so that you can see the fibres on it.

25 Now destroying the structure can be done with a little acetone. It's not hard to do. The problem is not to shrink it, not to change its dimensions, or not to have the fibres disappear down into the goo.

I don't see any reason why it would significantly alter the structure of the fibres.

30 If you're talking TEM, you get into the condensation, washing, and all that sort of thing, that there is some question whether you're gonna lose some, or I don't know how

5 A. (Cont'd.) much it would change it, but the losses are a question. The rub-out technique nobody, almost nobody uses any more.

DR. DUPRE: Q. So the preparation problem if it is there, is likely to be there in the TEM situation only. It's not a major factor.

10 DR. RHODES: A. It can be done, but there's no standardized procedure to do it. Every laboratory has their own favourite technique at this point in time.

Now if you're going to count at five thousand, you've got a whole different ballgame, you're counting different fibres. You get all kinds of numbers.

15 MR. LASKIN: Q. Is there a time problem in terms of analysing what you've got? I mean, can you do it as ...

DR. RHODES: A. No, it's a lot slower. On an optical microscope, with a good counter, he's going along there like he's playing a piano, plucking 'em off, and he's got a trained eyeball, and he can move that field very quickly, just turn a knob.

20 With an SEM, you've got all, it's like reading on a TV screen, and when you switch your view location, it takes a brief time for the picture to stabilize, so it's not anywhere as easy to pick up your fields.

Q. What about the day to day costs of samples? Is that a significant factor?

25 A. Well, the longer it takes you to count the sample, the more cost.

Q. You're paying for operator's time.

30 A. And you've got a much bigger maintenance problem on an electron microscope. You've got a high vacuum system that you've got to maintain.

Q. I'm sorry.

5 DR. CHASE: A. I'm just gonna throw in one point that was really I think, brought out earlier, this problem as you increase the magnification, if you were to double things, then it takes you four times as long to count the equivalent area, and it works that way.

10 MR. LASKIN: Q. If you use the same definitional rules of fibres, with SEMs with an optical microscope, can you with the SEM, can you still control for the fact that you can't measure, or you can't see fibres in an optical microscope that are thinner than a particular diameter?

I mean, can you control for that factor?

15 DR. RHODES: A. I'm not sure. Let me see if I understand your question. If you're looking through an SEM at five hundred mag, you see pretty much the same thing.

You get better resolution than you do looking at a membrane filter through a phase contrast.

20 Q. What I'm trying to put is, we've heard some evidence that for example, you might have a fibre under the optical microscope that'll meet the definition of an asbestos fibre, that is greater than five microns in length with a three to one or better aspect ratio, but you won't be able to measure it, because you can't see it, because it's too thin. It's below the resolution power of the optical microscope.

25 A. Well, your resolution is going to vary with the magnification you're working at.

Q. If you have the same, you know, the same field on the SEM, will you see that fibre? I mean can you ...

A. You'll see more fibres than you will with an optical microscope.

30 Q. And you're using fibres in the definitional sense?

A. Yeah, you can get your field, your resolution

5 A. (Cont'd.) is clearer on an SEM than in the phase contrast, but you're not going to get major differences in what you see.

DR. MUSTARD: Counsel, may I pose your question in a slightly different way?

MR. LASKIN: You certainly could.

10 DR. MUSTARD: Q. The optical microscope will not pick up the very thin long fibres, because of the - but, I'll make the statement, then you can say whether it's wrong or not.

On scanning electron microscopy, using a high enough resolution, we should be able to detect those fibres.

15 DR. RHODES: A. Using - well, if you're going to go up in magnification, yeah. I was making, what I was saying is, at the same magnification, there's not all that difference in what you can see. There is a difference.

Q. Except the resolution is that much sharper when you look down. You're going to see that you see it, and you automatically bring up the magnification. At least that's what I used ...

20 A. Yeah, well, if you're gonna allow yourself to move the magnification, yeah.

But you run up the big, you start scanning on the big mag and it's there, you're gonna miss, you have a tendency to miss the big fibres. You're taking a much smaller area.

25 I'm not knocking the SEM. I'm just saying it's a much more difficult instrument to work with than the optical microscope. I think it has its uses, but for massive routine sampling, it really adds to the burden.

MR. LASKIN: Q. Do you judge that it has its uses in the workplaces, in the asbestos workplace?

30 DR. RHODES: A. The place that I would say that it has its uses, is in the characterizations, selective use of

5 A. (Cont'd.) characterizations, where you have difficult dust clouds to work with. Dust clouds are not all created equal. There's some easy ones, and there's some difficult ones.

DR. MUSTARD: Can I, a question I was going to ask at the end, I may as well ask it now?

MR. LASKIN: Please do.

10 DR. MUSTARD: It seems to be the time.

DR. MUSTARD: Q. It's still unclear to this Commissioner what the most hazardous fibre form is, whether it's long and thin, whether it's just thin, or what it is, because, my problem is that the data that we have seen has not done what I would call the perfect experiment of taking perfectly defined
15 fibres, and experimentally finding out what they do.

It's all by association, and interpretation, and a large part of the fibre assessment appears to have been done with the optical microscope, which then has the problem that it only gives you a part of the story.

20 This really then raises an interesting dilemma for me in this whole business. If for example, in one of the processing areas, you tend to produce fibres, but very few thin fibres, but fibres of varying length, then the assessment that we're getting with the optical microscope is giving one story. But if in another process, you're not only getting that, but
25 you're also getting very thin fibres because of the fragmentation, your optical microscope then, is really giving you a different index than for the other place.

Now it would seem to me, that someone should take the scanning electron microscope, and attack that problem, because I believe that problem exists from the evidence we've had, about
30 the difference in fibre splitting. Do you know if anybody's tried to tackle that important issue?

DR. RHODES: A. I'm trying to think of a reference.
Do you know Gerry?

5 There's been, part of the DMAP work was trying to
get, collect information, available information on the subject of,
how different are the fibre sizes and diameters in point sources,
which is the same sort of thing, and I surveyed EPA and NIOSH,
and OSHA in some of the states, and I really didn't come up with
much.

10 DR. MUSTARD: Q. You see the dilemma, because I
could see that a two fibre per cc standard where you have very
few of these fibres, when you did the scanning electron microscopy,
might be a pretty good standard, whereas in another place, you
might want to drop it down to point five or point one, just
15 because of the fact that your optical microscope problems of
picking up the long thin fibres, so one is left with the problem as
to what that index is really reflecting, in terms of total fibre
mix.

A. I agree. There's not much I can say. There may
be a situation where certain types of operation would require at
20 least a characterization by more elaborate techniques, but it
really wouldn't make sense to make every operation go to more
elaborate analytical system.

It's a problem, I agree.

Q. No, and also, and even once you've standardized
that plant, you could then set what you thought the index would be
25 through a simpler technique from monitoring.

MR. LASKIN: Q. I've just got one other question,
and maybe you can help me with it.

30 It really has to do with evaluating a number of
the epidemiological studies that have come before us, and one of
the things that the Simpson report did, and you may be aware of it,
is made an adjustment because of the change from static to personal

5 Q. (Cont'd.) sampling, and in the Simpson report, there was an assumption that there was a factor of two involved, when you went from static to personal sampling, using the membrane filter method at about a two fibre standard. And I just wondered whether I could draw on your experience and knowledge to tell us whether, you know, that accords with your own experience, I mean, whether we ought to when we're evaluating all of these epidemiological studies that we've got, and trying to put some sense into the dose calculations, whether we should be making that kind of factor adjustment.

10 If I've taken you beyond your field, please tell me.

15 DR. RHODES: A. Well, I can comment in general terms, that much of the early data at Rochdale,

Correct me if I'm wrong, Gerry, was area samples, and they have operations where they're in effect, point sources, in the sense that the asbestos is being generated at the looms, and if you're taking samples back in the aisleways in-between them, there's going to be a rather substantial difference.

20 I will confess I have not read the backup article on the Simpson report, so I'm giving impressions.

I think a factor of two is not a bad order of magnitude theory - will be a very substantial drop. It could be more than that. It depends on the nature of the operation which is being sampled.

25 DR. DUPRE: Q. Now is the fact that two is not a bad one for that particular Rochdale plant, given what they are doing there, and what is going on near the looms ...

30 DR. RHODES: A. You're taking me ... you're taking me too far on that. I'm beyond the area that I really, I'm drawing on my experience, what I've seen on our area samples, and for a

A. (Cont'd.) small point source, a factor of two is much too small.

5 You can dump in a hood, and five or six feet away, it'll be down to a couple of tenths, so that you can get some very very sharp ratings.

Gerry, do you want to ... ?

10 DR. CHASE: A. Well I think yeah, that particular point becomes more critical as one moves back in time, because the way the dust clouds in the workplace have been reduced, have been by attacking point sources, and therefore you would have to attack this one, and then move over to the next worst one, and step around in that way, and therefore any kind of an area sample, that discrepancy could be greater with some of the older data, than it is now, where we're beginning to learn more about it.

15 And it just, you know, it becomes somewhat anecdotal when you begin to try and come up with a general factor, because certainly, if there's a point source, and you have an area sample, and you set it right next to it, and the worker's going outside of that region, then you could come up with a situation where that area sample would in fact be higher than
20 the point source.

But I'm told by industrial hygienists, they didn't, I mean if there was a point source, and there was a person had cause to be there, they didn't put the area sample there, because they would be, for one thing, tripping over it.

25 The sample was taken in the more general areas, so while anecdotally you can think of even reversing that thing, from a practical point of view, it certainly was the case that personal samples had we been taking them, would have been higher than the area samples, and you know, in that particular instance, a factor of two does seem to be a reasonable ballpark figure to
30 work with, but as Dr. Rhodes said, I think as you go for some

A. (Cont'd.) small point sources, for some situations, that factor could be substantially higher.

MR. LASKIN: Q. Or less.

DR. CHASE: A. Or less, you've got to run the whole spectrum.

Q. What you're really saying, is you've got to look at the particular establishment, and look at where the samples were taken from, and what kind of processes are going on, and how high the dust levels are.

A. Work practices, process changes, dust control, all of those considerations are the only way that one can come out to a reasonable informed estimate of a previous exposure.

DR. RHODES: A. Guesstimate might be a better term.

DR. CHASE: A. Well, you know, if we come, yeah, it's a guesstimate, and we do the best we can, and caveat the final answer. It's because of that need for knowledge of the process, and how the samples were taken, and what work practices may or may not have been implemented, and this sort of thing, why you have to supplement the, just the raw data in reconstructing past exposures.

Q. With the knowledge of the industrial hygienist as to what went on in the workplace.

A. Right, exactly.

MR. LASKIN: Thank you very much for bearing with me. I have no further questions, Mr. Chairman.

DR. DUPRE: Thank you, counsel.

Miss Jolley.

CROSS-EXAMINATION BY MISS JOLLEY

Q. I have very few questions, because they were mostly covered.

One of the things I was interested in, is the PAT

Q. (Cont'd.) NIOSH PAT laboratories, and I don't understand what the PAT laboratories are, or what this is.

5 DR. RHODES: A. NIOSH runs a program, a quality control programme for a number of various regulated contaminants, asbestos is one of them.

10 The programme consists of I think four samples a month, which they prepare. They prepare samples, which are all ostensibly the same, and they have some problems with getting exactly the same loading on all of the samples.

15 These are sent out to the laboratories, who then count them, and report the results back to NIOSH, who then does the statistical evaluation of the performance of the whole group, and of the individuals within the group, and they report back to you whether you are within acceptable counting limits.

20 Q. If the lab was found to be consistently out of line, would they then send their people into - say that it was a particular company's laboratory that was out of, are companies' laboratories part of this PAT?

A. Yes, anyone.

25 Q. Right.

And so if they were consistently out of line with the regular, would then NIOSH go in to see what they're doing wrong, or would the government go in?

30 A. No, the government would not intervene. The laboratory would be pretty much compelled to find out what was wrong. If they're an insurance company for example, they would certainly want to find out what their problem was.

Now NIOSH will provide assistance if you go and talk to them, but it's not a government control, it's a control quality assurance program which is run by the government which you may participate in, but they're not ...

35 Q. But you don't have to, if you're ...

DR. RHODES: A. You don't have to. There was one other point I wanted to make.

Go ahead, I've forgot mine.

DR. CHASE: A. PAT is for Proficiency Analytical Testings anachronism. It's a requirement that AIHA certified laboratories ...

DR. RHODES: Yeah, that was my point.

DR. CHASE: A. ... participate in the PAT programmes. Not all PAT participants are AIHA certified, but all AIHA certified laboratories, are PAT participants.

However, they do have the option of only doing some of the contaminants, the samples that are sent to them.

They don't have to, if a laboratory is never involved in evaluating asbestos samples, they don't, it's not necessary that they send those samples back.

But if that consistent bad performer, or the atypical, the outlier by their terminology, is, and this I could verify, but that threatens their certification ...

Q. With the AIHA.

A. That's right, yes, but I'm not sure of the mechanism, but I do know that as you, if you begin, I think it's like even two successive ones, or something like that, if you're the bad actor, if you're the atypical measurement that's way away from everybody else, then you're getting flagged, and being told about it.

Now I think that if someone who is a PAT participant who is not certified, I think then, it would be pretty well up to that, whatever use they're making of those data, would be up to them.

I don't believe that they would be kicked out of the PAT programme if they did that or not, because the PAT programme itself, analyses the data, and those that are the

A. Cont'd.) extreme cases, are thrown out when they evaluate, when they summarize.

5 DR. RHODES: A. Extreme results are thrown out. The people are not thrown out.

DR. CHASE: Yeah.

Q. Well, I think it's of some concern, because we're just getting into standard setting in Ontario, and it means a lot of industrial hygienists are now going to be, I mean I
10 understand a lot of companies have already been doing that kind of analysis. There'll be even more analysis done, and it's of concern how to somehow regulate the people doing the analysis.

DR. CHASE: A. The idea of you know, a comprehensive quality control programme like that, with all the caveats that go
15 into the problem, such as preparing those samples for sending out to the labs is just, you know is endorsed completely, and the PAT programme is certainly the best example of that, and I think as time goes on, as they've been able to improve the preparation of the samples, then I think you know, probably the analysis techniques will go on, and the uses that are made of those data
20 is an essential part of trying to pull everything in together.

As we talk about trying to standardize equipment and standardize performances, and stuff like that, that's one ongoing quality control mechanism that could be used.

MISS JOLLEY: That's the only question I have.

DR. DUPRE: Mr. McNamee.

25 CROSS-EXAMINATION BY MR. MCNAMEE

Q. Yes, just one or two questions, mainly a matter of clarification.

30 When Dr. Chatfield was here, I had understood from his evidence, that the fact that you can't see on a good

5 Q. (Cont'd.) quality optical microscope, even a trained observer can't see below about a two micron , point two micron diameter, and on a lesser quality microscope optical, it gets up to about point four microns.

10 I thought this was less of a function than the property of light, the wavelength of light, and really had very little to do with magnification, and do I understand wrong, that actually with an optical microscope, I thought even if you went to a two thousand power, that unless you had something to cause the fibre to be differentiated from the background environment, you couldn't see it.

Am I incorrect?

15 DR. RHODES: A. Well you're, on the phase contrast, your differential, your index and refraction is the thing that gives you the difference. I know that we look at nine hundred on some of ours, and we can see more at nine hundred than we can see at four hundred.

Maybe I'm wrong, I'm not a microscopist.

20 Q. You know, Dr. Chatfield indicated that if you use this carbon solution, that you could then see below the point two microns with the optical microscope, and that had something to do with the differentiation of refrac... or something like that, and I just didn't know. I thought it was less, more of a property of light than magnification, 'cos it's not the electron microscope that the ... it has something to do with X rays
25 knocking electrons off the object material.

Is that correct?

A. Yeah, you're getting a back-scattering basically.

30 Q. Yeah, and really it's not so much the quality of the vision, as it is of some, you know, this artificial ...

REPORTER'S NOTE: The cross-examination of Mr.

McNamee is barely audible, and odd words could be subject to question.

5 DR. RHODES: A. Well, I could be wrong, but my understanding, I know that when we go to oil immersion on our scope, we can see finer fibres than we can see without it.

Q. Oil immersion.

A. Oil immersion on the lense.

Q. Would that have something to do with the fact that you're creating a contrast with the background, and somehow making it change?

10 A. It may, yes.

Q. Well I guess it's difficult to ...

A. No, I'm sorry, I'm ...

Do you know, Gerry?

15 DR. CHASE: A. No, I would just say that there was a very detailed and thoughtful discussion along those lines at the first colloquium, dust colloquium at Warmen Steinach that we mentioned before, and it was done by Dr. ...

DR. RHODES: Leguen, Roger Leguen.

20 DR. CHASE: A. Leguen, from Britain, who's with the, in the factory inspectorate research laboratories.

Q. You see, the way I think is, that two people looking through a four hundred power optical microscope, that a person with very sharp eyes, if it weren't for this property of light, the wavelength of light that a person with very sharp eyes might see something one tenth of a micron.

25 Is there something wrong with the eyeball?

DR. RHODES: A. No, they do.

Q. They do.

30 A. The DMAP has a series of slides with polystyrene spheres on them, and they have set a range of approximately two tenths to point two six as the proper alignment and optical setup for counting fibres. There are people I'm told, I can't, haven't seen it personally, who can see the little spheres down

A. (Cont'd.) around a tenth.

Q. So then the eye isn't limited to point ...

5 A. The eye, the eye is a critical part of the looking through the optical microscope.

Do you have a counter?

I've been told that there are counters who can see down to a tenth, as rare birds. Most of the scopes I've seen, are around the two tenths, three tenths, and this is ...

10 DR. CHASE: A. ... Built in limitation to the eye mechanism, below a certain ...

DR. RHODES: A. It's involved with the light, and, the eye and the light. I'm sorry, I'm just not that much of an optical type.

15 MR. MCNAMEE: Thank you very much.

DR. DUPRE: Dr. Uffen.

DR. UFFEN: Q. I've just got one question, and I'm not even sure how to pose it.

20 One of the problems we have to pay attention to, is what about the workman who is subjected to short, but intense exposure, in the renovation and demolition industry?

We've been talking about the membrane type of method. Is it the right thing to use in those circumstances?

25 The conditions are different, see, the workman, he's the first one to run into it. The boss or foreman may not show up for a couple of hours, and he might be in quite an intense cloud for a little while, and I'm thinking in terms of a technique that the workman can use. You don't have to have an industrial hygienist to do it. He can tell that he's being exposed, and withdraw until something more elaborate is done.

30 DR. CHASE: A. Are you getting at the work practice type of thing?

A. Yeah.

5 DR. CHASE: A. There are certainly situations where the work practice is I think, far more practical for the routine control of the exposures, than through the monitoring, but of course at the heart of that, would also be the membrane filter technique in terms of establishing that work practice.

10 One would want to come up with a work practice that reliably kept exposures down, and the way that one could do that, is under, if you will, more controlled conditions, to establish that the work practice does in fact, keep those down.

15 So that I think that in terms of monitoring, that the membrane filter would have a use, but it comes in the establishment of the work practice, and then, of course, once the work practice is established, then that would be the procedure to be followed.

20 On the other hand, when you talk about the detection, we don't have the analogous canary, who will collapse when ...

25 DR. UFFEN: Q. When Mr. Trudeau was here, he talked to us briefly about a bunch of different other instruments, and I think he was pretty frank about some of them

One of them he said was useless, because it wouldn't work. It had to be on a tripod, like a surveyor's instrument. The vibrations from the plant made it useless.

30 Dr. Rhodes I guess, it's to you that I'm - in your practical experience, are you familiar with any other instruments that could be used for this short intense exposure problem?

35 DR. RHODES: A. I don't know, well, let me say this. First of all, when you start trying to use the membrane filter for very very short time periods, it really gets erratic on you. It tends to give you some very high numbers, and also some very low numbers, and just why, I don't know.

A. (Cont'd.) It gets erratic.

5 All of the instruments that I know of, the
continuous reading instruments, require a calibration. You have
to calibrate them against a counter filter, and the calibration
is unique to the particular type of dust that you're trying to
count. So the use that I see for these continuous monitoring
instruments, is basically engineering controls, where you've
10 established the characteristics of your dust cloud, and you
want to watch how your control devices are working controlling
it.

15 They use them quite a bit in the asbestos cement
industry in Europe, for one thing, to watch what's going on,
rather than as a primary method of determining airborne asbestos
concentration.

20 Now the problem of applying that to your
particular situation is one that EPA has been grappling with
now. You've got a material here on the wall. It could have
anywhere from no asbestos, to seventy or eighty percent, to pick
a number. To try and find a way to characterize the material
that you're working on, has not been solved, I don't think, and
you certainly don't want to require that - there's no quick way.
You can put a, you put one of these instruments in on a dust
cloud, and you get a reading.

25 There's no way of saying how much of that is
asbestos, and how much is something else.

I think in answer to your question, I know of
no way to get a quick asbestos reading.

DR. UFFEN: Q. Even a rough and ready ...

30 A. Not even a rough and ready. Rochdale has a
thing that they've got calibrated very extensively for their
straight asbestos dust clouds, but as far as sticking it, using
it for an unknown dust cloud, it's just useless.

DR. DUPRE: Q. I just have one line of questioning.

I very much appreciate the extent to which, what you've given us today, both orally, and in this written statement is by way of some very very practical guidelines for the jurisdiction with which to consider certain kinds of standards in asbestos regulations, and they were put out in a very simple context, I guess.

I think that one thing that you would have been telling me here if for example, the Simpson report was being implemented, and I was the unfortunate, who had been designated as in charge of implementation, that as far as their recommended standard of one fibre for chrysotile is concerned, the membrane filter method appears to be up to snuff, in terms of reasonable confidence that both the employer, and the inspector would have it.

Now the problem at this point is that there's another recommendation in the Simpson report, which is, that where amosite is concerned, we're going to go for a point five fibre standard.

Now I fully appreciate the extent to which of course you have said so clearly, your Round Robin study is into the chrysotile area by and large, and certainly the lessons to be drawn from it could be, I take it, read as lessons in the context where whatever the standard, there's only one asbestos type, but if I as a would-be implementor am stuck, let us say with a regulatory system that is going to try to regulate at different standards, two or more asbestos types, are there some practical messages that I could take out of this?

DR. CHASE: A. Are you discussing in the context of the specific numbers in the Simpson report, the point five, or point one, or is it the, is there anything ...

DR. DUPRE: Q. Well, if at the point five level,

5 DR. DUPRE: Q. (Cont'd.) one starts to push the capacity of the figure of a particular method, so that much I'd have clear, although I might have to go ahead and do something about it anyway. So we avoid that problem. Supposing I was instructed to implement a duel standard system, where all right, one is two, and the other is one fibre, so that the second type of asbestos is at your one fibre count, membrane filter threshold I think you called that, would you have any particular practical
10 observations to give me as to what my problems would be?

15 DR. CHASE: A. Well, are you going to say, in the situation you just descibed, are you going to say if the amosite is there, it's one for everything, or it's just, I know you didn't mean this, but let me just take the extreme case. You can have two for the chrysotile, and one for the amosite, totalling up to three. I know you don't mean that, but so what would be the permissible? Could you have, if the chrysotile was say, say half of the dust cloud, does that allow you to total up to one and a half?

20 Q. Well, I don't know, because maybe you can answer me.

A. Maybe I'm asking you the question you're trying to ask me.

25 Q. O.K., you're trying to ask me a question, and I will give you an answer to your question. That's in terms of what - a clue that I think you've given me in here, and I think the clue you've given me in here, is the following.

30 If I'm in charge of implementing this regulatory regime with a duel standard for two different asbestos types, I guess obviously, the first thing I'm going to do to try and keep things simple, is I would be of course, trying simply on the basis of what is known of our plants, naturally to isolate the hundred percent chrysotile plants from the hundred percent

5 DR. DUPRE: Q. (Cont'd.) amosite plants, because that's the easy part. Once you know it's a hundred percent of one fibre or the other, at least you've got a start.

Now my in-between problem of course, is going to have to, is necessarily going to focus on all of those plants in which there is a mixture of two or more types of asbestos, the relative proportions of course, perhaps unknown.

10 Now given I think one of the hints you've given me here, I guess that the first thing I would be tempted to do, is to go to electron microscopy. It may be SEM would be adequate for the job, to at least give me some kind of a quantitative handle on the workplace dust cloud situations in my different plants.

15 And so, you know, from this, I might be able to point, to find out with what degree of confidence, heaven knows, maybe you can tell me, but if it's you know, in one plant it's two thirds amosite, one third chrysotile, and another plant it's some other proportion.

20 Now let's just say, to make things really easy, that O.K., I've got one plant where through electron microscopy, I have reasonable confidence as the regulator. For that matter, the employer agrees. O.K., when all is said and done, it's two thirds chrysotile, and one third amosite. O.K., now of course, we get into the business of applying our two standards, which means of course, if we're going to, I assume, because of the practical nature of the situation, follow a membrane filter technique, with personal sampling, and so on.

25 Now is it as crude as the following? That you get a fibre count, you know, you get your fibre count off your membrane filter method, and you always assume given what SEM has told you, that in any given sample, it's going to be roughly
30 two thirds of one, and one third of the other, or do you run, can you actually ask your counters to report a count of amosite fibres,

DR. DUPRE: Q. (Cont'd.) and of chrysotile fibres? And what kind of random errors, what kind of random error escalation are you going to get if you do that?

DR. CHASE: A. I don't have the answer in terms of what that cloud would vary from over days in terms of that percentage make-up, but to take your example, and say suppose we could document with some degree of reliability how much that percentage varied, or rather conversely, how constant it was, then if you could come up with a quantification of that percentage variation, then the idea of assuming that in the future, would not be again, with the appropriate - analogous to what we were doing before, using the statistics to try and come up with some point that would be a reasonable upper bound, or estimate, or average, or whatever it is that would be deemed appropriate for that situation.

Then to use that in the future, provided there were no process changes and this sort of thing, I would feel comfortable with, because you're again using the statistics to, the empirical information, to quantify the uncertainty in terms of the judgement you're making.

To take the one situation that you said, I would feel comfortable with that, as long again, you'd have to work out the specifics of it, say well I think we - would you take if you had two thirds, the example of two thirds amosite, and one third chrysotile, that you would then, taking your two and one, that you would then take two thirds of one, plus one third of two, giving you a one point three three, and that gives you your total.

That is certainly feasible from the perspective that I take right now, without having empirical information to do it.

DR. DUPRE: Q. But now O.K., now just in terms of

5 DR. DUPRE: Q. (Cont'd.) a practicality of getting
the empirical information, if your recording machine
is differentiating between asbestos types, is electron microscopy
really the only reliable way of getting a handle on that piece
of empirical information you need, of what proportion is of one,
and what proportion of the other, or is the optical method
including the capacity of a ?? to the count, up to giving
you a count of so many brown fibres, so many white fibres, or so
10 many amosite fibres, so many chrysotile fibres.

DR. RHODES: A. Unfortunately, they're not brown
white, blue. Chrysotile on phase contrast, even the small ones
down around five microns, has a look about it that an experienced
counter tends to recognize. I emphasize tends to recognize.

15 Q. It's the dimension he tends to recognize.

A. Well, it's the general look of the stuff. It
tends to be a little hairy. It's got some characteristics that
you learn to recognize. Amphiboles, and I'm going to emphasize
our experience has been very very limited with these, amphiboles,
and other minerals all, they don't have this type of species
20 identity. Amphiboles can look like other mineral species, and
it's much harder if you've got a mixed dust cloud to distinguish
those, than it is to make a pretty good guess that it's
chrysotile.

Now in our laboratory, where we have been only
interested particularly in chrysotile, we're looking at
25 applications where chrysotile has been added, and we're looking
primarily at the impact of the chrysotile exposure. We report
chrysotile and possible amphibole, which is a unique - I think
we're probably the only laboratory that does it, and we say
possible amphibole, because there's so many things that look like
amphibole asbestos, and I'm telling you this to emphasize that
30 as soon as you get over into the other areas, other types of

5 A. (Cont'd.) asbestos, you have much more trouble distinguishing it from other background mineral fibres in amongst them, than you do with chrysotile.

10 Now there are some techniques polarized light, dispersion staining, refractive index extinction angle, that sort of thing, that a really experienced microscopist can do a lot with, but these guys are about as scarce, probable more scarce than trained electron microscopists, so that you get - really, electron microscopy is the only way that I know of to try and get yourself a characterization level.

15 DR. DUPRE: Mr. Sampson, do you have any final questions of your witness?

MR. SAMPSON: No.

20 DR. DUPRE: Well Dr. Chase, Dr. Rhodes, you've been most helpful. May I thank you on behalf of all of us.

Before we adjourn Miss Kahn, you have I believe, a memo on our forthcoming schedule.

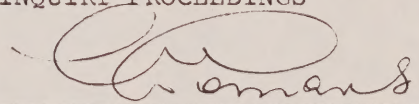
25 MISS KAHN: I believe it's already been distributed. I think everybody in the room has one.

30 DR. DUPRE: So I take it that we have, all of us, some kind of an idea of where we're going for the rest of the month, and in any event, that I can with confidence, say that we now rise until ten a.m., next Thursday, which is also the twentieth of August.

Thank you indeed.

THE INQUIRY ADJOURNED

THE FOREGOING HAS BEEN PREPARED
FROM THE TAPED RECORDINGS OF
THE INQUIRY PROCEEDINGS



C. LESLIE HOMANS

